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Psychological Review

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JOHN B. WATSON, Johns Hopkins University (*J. of Exp. Psychol.*)
JAMES R. ANGELL, University of Chicago (*Monographs*)
SHEPHERD I. FRANZ, Govt. Hosp. for Insane (*Bulletin*) and
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VOL. 25. No. 2 March, 1918

THE PSYCHOLOGICAL REVIEW

PSYCHOLOGY IN RELATION TO THE WAR[1]

BY MAJOR ROBERT M. YERKES,

Sanitary Corps, National Army, Surgeon General's Office, Washington, D. C.

It is my agreeable duty as President of the American Psychological Association to present to you in outline the history of the organizing of psychological military service. I shall limit myself strictly to the activities of the present year, 1917.

In Europe, psychologists have served conspicuously in the great war but psychology has done little. In this country, for the first time in the history of our science, a general organization in the interests of certain ideal and practical aims has been effected. Today American psychology is placing a highly trained and eager personnel at the service of our military organizations. We are acting not individually but collectively on the basis of common training and common faith in the practical value of our work. At the first call American psychologists responded promptly and heartily, therefore the length to which the development of our work has progressed and the measure of service which has been attained.

On April 6, 1917, in connection with a meeting of a group of psychologists known as the "Experimentalists" which was at the time being held in Emerson Hall, Cambridge, Massachusetts, a session was arranged by Messrs. Langfeld and Yerkes with the approval of the chairman of the meeting, Mr. Titchener, for discussion of the relations of psychology to national defense. Captain Bowen, instructor in military

[1] Address of the President before the American Psychological Association, Pittsburgh Meeting, December, 1917.

science and tactics, Harvard University, attended this meeting and made valuable suggestions concerning the possible rôle of psychology. Notable suggestions were made also by Messrs. Bingham, Troland, Langfeld, Doll, Reeves, Burtt, Wells and others. At the conclusion of the discussion, which clearly indicated confidence in the preparedness of psychology for military service, it was moved by Mr. Warren that a committee consisting of Messrs. Yerkes, Bingham and Dodge be appointed to gather information concerning the possible relations of psychology to military affairs and to further the application of psychological methods to military problems.

On the evening of the same day at an informal conference of the members of this committee (Mr. Bingham's place being taken by Mr. Ogden), it was decided that the matter should be placed before the council of the American Psychological Association, so that our national organization, rather than any restricted or local group, might take action. Following this conference, the president of the American Psychological Association prepared the following letter, which on April 7 was dispatched to the members of the council of the association.

EMERSON HALL, CAMBRIDGE, MASS.
April 6, 1917

TO THE COUNCIL OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION,

Gentlemen: In the present perilous situation, it is obviously desirable that the psychologists of the country act unitedly in the interests of defense. Our knowledge and our methods are of importance to the military service of our country, and it is our duty to coöperate to the fullest extent and immediately toward the increased efficiency of our Army and Navy. Formalities are not in order. We should act at once as a professional group as well as individually.

As president of the American Psychological Association, I apparently have choice of two lines of action: either I may recommend to the council that a special meeting of the association be called at once to consider the general situation, or I may, instead, ask the council to authorize the appointment, by the president, of such committee or committees from the association membership as seem desirable.

After consultation with a number of members of the association,

I have chosen the second alternative, and I hereby request the council's authorization to appoint such necessary and desirable committee or committees.

The duties of any group or groups of members appointed to represent and act for us would evidently consist, first, in gathering all useful information concerning the varied aspects of the actual and possible practical relations of psychology to military affairs; second, to coöperate, as circumstances dictate, with governmental agencies, with the National Council of Defense, with local psychological groups or individuals, and with such other agencies as may develop; third, to further the development and application of methods to the immediate problems of military selection.

Already many of us are working for national defense in our respective communities. It is my thought that this action by our council should, far from interfering with individual initiative, tend to unite us as a professional group in a nation-wide effort to render our professional training serviceable.

I urge you, gentlemen, to give this matter your immediate consideration, and I trust that you will write freely concerning your own activities, plans and opinions, for your advice and suggestions concerning all aspects of the problem will be quite as welcome as your vote on the above recommendation.

Yours faithfully,

ROBERT M. YERKES,

President, American Psychological Association

It was deemed desirable by two members of the council that a meeting be called immediately for consideration of the situation and decision concerning desirable action. Such meeting was called by the president in Philadelphia on the evening of April 21.

In the meantime the writer, in order to obtain pertinent information concerning actual and possible applications of psychology to military problems and, by request, to advise the Military Hospitals' Commission of Canada concerning the use of psychological methods, proceeded to the Dominion of Canada and made careful inquiry concerning psychological activities in Montreal, Ottawa and Toronto. The following memorandum report of observations in Canada was prepared especially for the Council of the American Psychological

Association. It will serve to indicate both the general nature of the writer's inquiries and information and the personal sources of the same.

Memorandum of Trip to Canada, April 10–14, for Consultation with Military Hospitals' Commission Concerning Applications of Psychological Methods to Soldiers

From Boston to Montreal, Tuesday night, April 10. On Wednesday, April 11, called on Professor William Caldwell, McGill University, and after conference with him was introduced to Major Muchleston, with whom I talked concerning the use of psychological methods in examining recruits. Later lunched with Major Muchleston and Captain Robertson at University Club, and discussed with them possible relations of psychology to varied aspects of the military situation. Also, received the opinions of Captain Ross, a returned officer, who had been wounded in France and who was strongly of the opinion that psychologists and psychiatrists should be of great service in the present situation.

Left at four o'clock for Ottawa, arriving about seven-thirty. Was met by Messrs. E. H. Scammell and T. B. Kidner, of the Military Hospitals' Commission staff, and also by Captain Farrar and Doctor Brigham. We later for two hours discussed the organization and psychological relations of the commission.

Thursday morning, April 12, I spent two hours in conference with Doctor Brigham, describing tests in use at the Psychopathic Hospital, Boston, and offering suggestions concerning their application to the military hospitals situation. The remainder of the morning was spent at the offices of the commission, in discussion of various psychological problems with the staff and in the examination of various reports concerning the condition and treatment of incapacitated soldiers.

Lunched at the Rideau Club with Messrs. Scammell, Kidner, Farrar, Brigham and W. L. McKenzie King. From this group I received valuable information. One of the most

impressive things about the conversation was the constant emphasis of the spiritual as contrasted with the material in human life and the evident feeling that for the sake of material development certain essential aspects of the human have been neglected.

The afternoon was spent at the offices of the commission in further analysis of data and study of the relation of the organization to psychological methods and in conference with Senator McClennan, a member of the commission.

Later, Doctors Farrar and Brigham took me to the office of Major Dodd, who as member of the Board of Pension Commissioners, has very evident appreciation of the need of psychological information, but, like most of his colleagues, is unable to obtain the desired data.

The evening was spent with Doctors Farrar and Brigham in further discussion of the relations of psychologists to the problems of mental disease and defect as they appear in soldiers who are discharged from the army.

Lieut.-Colonel Alfred Thompson was absent from Ottawa, but I met and conferred with Captain Chipman and various civilian authorities connected with the Military Hospitals' Commission and obtained printed materials and various typewritten reports.

The general impression which my conferences in Ottawa gave me was that of urgent and rather generally appreciated psychological needs.

At eleven o'clock Doctor Brigham and I left for Toronto. Arriving there in the morning, we got in touch with Captain Ryan, of the Military Hospitals' Commission, and were taken by him to Doctor Clark, of Toronto University, who is superintendent of the university hospital and dean of the medical school. He, after telling us of the psychiatric work in his institution and of his interest in the problem of feeble-mindedness, took us to the psychological laboratory of the University of Toronto, and there introduced us to Mr. Bott, who exhibited and explained to us most varied and interesting forms of home-made apparatus for the reëducation of crippled soldiers. The entire laboratory is given up to this work,

and Mr. Bott, together with a number of assistants, gives practically all of his time to the task of reëducation.

This work is evidently the most important, from the social and economic standpoints, necessitated by the great war, since hundreds of thousands of soldiers are being returned to society partially incapacitated by paralysis or by the loss or injury of members. The relations of psychology to methods of reëducation, and in general to the treatment of these victims of war, are numerous and obviously important, but at present too little is being done in Canada or elsewhere toward utilizing either psychological knowledge or technical psychological methods.

After lunching with Doctor Clark at the university hospital, we were taken by Captain Ryan to visit two military hospitals. During this trip I especially inquired about the examination of recruits for the aviation corps and learned that no especially significant psychological tests are included in the examination.

In the military hospitals, which for the most part are buildings temporarily converted for the purpose, we saw in a general way the facilities for after-care and treatment of the men who are being returned to the Dominion now at the rate of about one hundred per day.

The chief work of the Military Hospitals' Commission, as indicated by its bulletins and other reports, is the proper distribution and medical care of returned soldiers and their vocational reëducation.

At five o'clock Friday, April 13, I left Toronto for Philadelphia. On the way I had opportunity to examine my Canadian materials and notes and to make memoranda concerning my observations.

While in Ottawa the writer received a telegram from Dr. George E. Hale, chairman of the National Research Council, requesting a conference in Philadelphia on April 14. In accordance with this request the president of the association met Dr. Hale and briefly reported to him the action which had been taken by American psychologists and the results of

observations in Canada. President Hale requested that a psychology committee be organized in connection with the National Research Council and that the president of the American Psychological Association act as chairman of the committee and as member of the council.

He further invited the president of the association to attend the semi-annual meeting of the National Research Council in Washington on April 19, as a representative of psychological interests.

At a special meeting of the council of the American Psychological Association which was held on the evening of April 21 and the morning of April 22 in Philadelphia, there were present, in addition to the president, the secretary of the association, Mr. Langfeld, Messrs. Angier, Bingham, Dunlap and Scott of the council, and by invitation Mr. Dodge. Messrs. Carr and Hollingworth were unable to attend. The action of the council upon the original recommendations of the president are recorded in the following minutes of the meeting.

Minutes of Special Meeting of the Council of the American Psychological Association

The president reported his investigations concerning the possibility of the coöperation of psychologists in a scientific capacity in the present emergency. He described his trip to Ottawa, Toronto, and Montreal, where he found the authorities very much interested in the possibility of psychological assistance. His impression was that they realized that they had made a mistake in not using psychological methods for the selection of recruits and for reëducation from the beginning of the war. The president later went to Washington to consult with the National Research Council.

It was voted that the president be instructed to appoint committees from the membership of the American Psychological Association to render to the government of the United States all possible assistance with psychological problems arising from the present military emergency. The following committees were authorized and their chairmen named.[1]

1. Committee on psychological literature relating to military

[1] For membership of committees see "Psychology and National Service," Psychol. Bull., 1917, 14, 259–262.

affairs. A bibliography should be prepared and the important pertinent literature digested, so that the desirable information may be available to individuals who are interested in various lines of service or are undertaking the solution of special problems.

Chairman, Professor Madison Bentley.

2. Committee on the psychological examination of recruits. It is necessary to prepare a plan for this task, to arrange methods of examining, and if arrangements are made with the War Department, to organize a corps of examiners.

Chairman, Professor Robert M. Yerkes.

3. Committee on the selection of men for tasks requiring special aptitude, as for example various kinds of artillery service, signaling, etc. Chairman, Professor E. L. Thorndike.

4. Committee on psychological problems of aviation, including the pertinent literature, the psychological classifications of an aviator, and the relations of these classifications to mechanical problems. Chairman, Doctor H. E. Burtt.

5. Committee on psychological problems of incapacity, including those of shell shock, reëducation, etc.

Chairman, Doctor S. I. Franz.

6. Committee on psychological problems of vocational characteristics and vocational advice. These problems are related to those of reëducation and incapacity.

Chairman, Professor John B. Watson.

7. Committee on recreation in the army and navy.

Chairman, Professor George A. Coe.

8. Committee on the pedagogical and psychological problems of military training and discipline.

Chairman, Professor Charles H. Judd.

9. Committee on problems of motivation in connection with military activities. Chairman, Professor Walter D. Scott.

10. Committee on problems of emotional characteristics, self-control, etc., in their relations to military demands.

Chairman, Professor Robert S. Woodworth.

11. Committee on acoustic problems and characteristics of the sense of hearing in relation to military service; for example, the

significance of localization of sounds, auditory acuity, the possibility of developing ability to discriminate different projectiles by their sound, etc. Chairman, Professor Carl E. Seashore.

12. Committee on problems of vision which have military significance. Chairman, Professor Raymond Dodge.

It was voted that, in order to provide the necessary funds for the development of the plans for national service, the council instruct the president of the association to appoint a special finance committee of three, which shall be empowered to raise and disburse a special fund, without authorization of the council. If the fund is not sufficient, said committee shall be empowered to present itemized bills, not to exceed the sum of one thousand dollars, which the council at the next annual meeting of the association shall recommend to be paid from the funds of the association.

It was moved and seconded that the secretary be instructed to formulate a letter to the members of the association describing the action of the council at this special meeting, in the hope that the members will communicate with the president concerning the best methods of offering their own resources and the resources of their laboratories to the government.

The council made certain suggestions to the president concerning the presentation to the proper government authorities of a plan for the psychological examination of recruits, and authorized the president to proceed with such presentation.

HERBERT S. LANGFELD,
Secretary, American Psychological Association.

Following this council meeting and by authorization already indicated, a psychology committee of the National Research Council was organized with the following membership: Messrs. Cattell, Hall, and Thorndike from the National Academy of Sciences; Messrs. Dodge, Franz and Whipple from the American Psychological Association; and Messrs. Seashore, Watson and Yerkes from the American Association for the Advancement of Science.[1]

At the first meeting of this committee, it was voted "that whereas psychologists in common with other men of science may be able to do invaluable work for national service and in

[1] Mr. Cattell resigned from the committee in October. Messrs. Angell and Scott have been added to the membership.

the conduct of the war, it is recommended by this committee that psychologists volunteer for and be assigned to the work in which their service will be of the greatest use to the nation. In the case of students of psychology, this may involve the completion of the studies on which they are engaged."

It is the function of this general committee to organize and, in a general way, supervise psychological research and service in the present emergency. Problems suggested by military officers or by psychologists are referred by the committee to appropriate individuals or institutions for immediate attention.

Several of the committees originally appointed by the Council of the American Psychological Association were subsequently accepted as subcommittees of the psychology committee of the National Research Council.

After the meeting of the National Research Council which the president of the American Psychological Association was privileged to attend in Washington, and at which he made a brief statement concerning the possible service of psychology to the military organizations, a circular letter was addressed to the members of the American Psychological Association in which their coöperation with the government in the interest of national defense was suggested. It was especially indicated that psychological laboratories might be made available and that offers of personal service would materially assist the council in formulating and furthering plans for the development of national service.

During the last week in April, in pursuance of the suggestions of the council of the American Psychological Association, the president acting as chairman of the committee on methods for the psychological examining of recruits, prepared for transmission to the proper military authorities the following plan for the examining of recruits.

Plan for the Psychological Examining of Recruits to Eliminate the Mentally Unfit

Whereas the Council of the American Psychological Association is convinced that in the present emergency American

psychologists can substantially serve the government under the medical corps of the Army and Navy by examining recruits with respect especially to intellectual deficiency, psychopathic tendencies, nervous instability, and inadequate self-control, it has voted to present to the proper military authorities the following plan and suggestions for psychological service.

This is not intended as a reflection on the work of the military medical examiner, but instead as an offer of special professional aid in a time of unusual strain, pressure and haste. Psychologically incompetent recruits are peculiarly dangerous risks with respect to disaster in action, incapacity, and subsequent pension claims. For this reason and because few medical examiners are trained in the use of modern methods of psychological examining, our profession should be of extreme value to the medical corps.

It is proposed:

1. That the psychological examining be conducted in the training camps and, if possible, before men are finally accepted for the service. It is assumed that there will be ten to fifteen thousand men per camp.

2. That a chief psychological examiner, who shall be also an officer in the Medical Reserve Corps[1] and responsible to the chief medical officer, be located in each camp, to organize, direct and engage in the psychological work.

3. That the chief psychologist be allowed an assistant examiner for each twenty-five hundred men in the camp.

4. That each assistant psychological examiner be given a commission in the Medical Reserve Corps, in order that the psychological work may be conducted with proper decorum and with due respect of private for examiner. The service of civilians as psychological examiners would appear to be undesirable.

5. That properly trained privates, non-commissioned and commissioned officers, be assigned to psychological examining as need requires and given rank in the Medical Reserve Corps.

[1] Since most psychological examiners do not hold medical degrees, it would be necessary probably to commission them as civilian experts under the Medical Corps.

This provides for the contingency that a large number of the men who are especially qualified for this work may have enlisted or accepted commissions in army or navy prior to the organizing of psychological corps.

6. That an appropriate private examining room with floor space of approximately 10 by 12 feet be provided for each psychological examiner. For a camp of ten to fifteen thousand men, five such rooms would be required. A table, flat-top desk with locked drawers, and two or three chairs would be needed in each room.

7. That the necessary materials for psychological examining and the requisite record blanks and filing cards be supplied by the Government. It is estimated that the materials would cost approximately five dollars per examiner, wholesale, and the record sheets not more than a cent per man examined.

8. That the procedure of examining be substantially as follows:

(*a*) In consultation with medical officers and company officers the psychologist should prepare a list of all men in a given company for whom special psychological examination is indicated by exceptional or unsatisfactory behavior.

(*b*) With this special list before him the psychological examiner should summon the men of the company to appear, one at a time, in his examining room. Each should there be subjected to a short series of mental measurements, the necessary time for which should not exceed ten minutes. The result of these measurements should be a rough estimate of the mental status and chief characteristics of the individual and consequent classification as mentally *inferior*, *normal*, or *superior*. Special attention should be given to men whose mental fitness had been questioned by medical or other officers.

(*c*) The normal group (probably 80 to 90 per cent. of all) should be passed, without further examination, as mentally competent. The inferiors should be given a special examination at once to decide whether they should be eliminated from the service. The superiors, time permitting, should be systematically examined for indications of their special value in the military organization.

(*d*) The special examination for inferiors (or superiors) would

require from thirty to sixty minutes. It should consist of measurements of various forms and aspects of response, among which should appear motor characteristics (for example, quickness, steadiness, and fatigability), observation, memory, suggestibility, adaptability or rapidity of learning, judgment, reasoning power, instinctive and emotional traits.

(*e*) The psychological staff should discuss the examination record of each inferior man and vote on the question of recommending to the medical officer his rejection or discharge from the service.

9. Further, this plan should be elaborated and perfected by a competent group of psychological examiners in conference and in coöperation with military authorities.

ROBERT M. YERKES,
Chairman, Psychology Committee,
National Research Council.

Early in May this plan was submitted to the president of the National Research Council, who in turn referred it to the chairman of the Committee on Medicine and Hygiene of the Council, Dr. Victor C. Vaughan. With Dr. Vaughan's support and coöperation the plan was promptly placed before the Surgeon General of the Army.

The evident necessity for developing methods of psychological examining especially adapted to military needs stimulated the chairman of the committee on methods of examining recruits to seek such financial aid as would render possible the organizing of an active committee for this special task. About the middle of May this need and opportunity were brought to the attention of the Committee on Provision for the Feebleminded (Philadelphia), whose secretary, Mr. Joseph P. Byers, immediately presented the matter to his board. It was promptly voted by this organization to offer the committee on methods facilities for work at The Training School, Vineland, New Jersey, and to meet the expenses of the work to an amount not to exceed five hundred dollars. This sum was later increased to seven hundred dollars. On the basis of this offer of assistance, a committee consisting of Messrs. Bingham, Goddard, Haines, Terman, Wells, Whipple

and Yerkes was assembled at The Training School, Vineland, New Jersey, on May 28. It remained in session until June 9 when it adjourned for two weeks to make certain trial of methods which had been devised.

During the first two weeks it was decided to arrange a method of examining recruits in groups of twenty-five to fifty, as an initial psychological survey. The group method, as finally agreed upon and printed for preliminary trial, consists of ten different measurements.

From June 10 to 23, the various members of the committee conducted examinations by the above method in several parts of the country. In all, about four hundred examinations were made, chiefly upon United States Marines and candidates in officers' training camps. These measurements were carefully analyzed by the committee and used as a basis for revision and the devising of methods of scoring.

On June 25, the committee resumed its sessions at Vineland and continued its work until Saturday, July 7, when it adjourned, on the completion of tentative methods of group and individual examining. At this time the committee had in press five forms of group examination record blanks; an individual examination record blank, which provides special forms of measurement for illiterates, those who have difficulty with the English language, those who exhibit irregularities suggestive of psychopathic condition, those who are intellectually subnormal or inferior, and finally, those who are distinctly supernormal; an examiner's guide, which contains directions for the conduct of examinations; and various types of special record sheet.

Before its adjournment, the committee, through a joint committee of psychiatrists and psychologists, consisting of Doctors Copp, Meyer, Williams, Terman, Haines (Bingham, alternate) and Yerkes received assurance from the committee on furnishing hospital units for nervous and mental disorders to the United States government[1] that the above committee would finance to the extent of twenty-five hundred dollars the trial of the above methods of psychological examining in

[1] Mental Hygiene War Work Committee.

various army and navy organizations, the work to be so planned as to test thoroughly the reliability and serviceableness of the methods and to supply materials for their improvement and for the development of satisfactory methods of scoring and reporting data of examinations.

This offer of assistance resulted in the prompt formulation of the following plan, which was successfully carried out.

Outline of Plan for Trial of Methods of Examining Recruits

Five groups of three men each to be organized for immediate work in four different military establishments, each group to consist of a chief examiner and two assistants. The fifth group to be organized for statistical work.

The four examining groups are to work for one month in either naval stations, army reorganization camps, or officers' training camps. It is proposed that approximately one thousand men be examined at each place by the group method, and approximately two hundred by the individual method. Further, that so far as possible, the results of these examinations be correlated with industrial and military records or histories.

This work is to begin as soon after July 15 as possible. Toward the expense $2,500 has been appropriated.

The estimated cost of the work is as follows:

Printing	$ 500.00
Stencils	25.00
Maintenance and travelling expenses of fifteen men, $75 each	1,125.00
Examiners' materials, including lock-boxes for shipment and storage	290.00
Furnishings for four examining rooms	250.00
Total	$2,190.00

Records of examinations are to be shipped to the statistical unit in New York City as rapidly as possible, so that they may be scored and the results evaluated and correlated with a view to determining the best methods of scoring and desirable changes in methods of examining.

Personnel of Units

Examining unit, Fort Benjamin Harrison, Indianapolis, Ind.: Chief examiner, G. M. Whipple, succeeded by T. H. Haines; Assistant examiners, J. E. Anderson, W. K. Layton.

Examining unit, Camp Jackson, Nashville, Tenn.: Chief examiner, E. K. Strong; assistant examiners, B. R. Simpson, D. G. Paterson.

Examining unit, Reorganization Camp, Syracuse, N. Y.: Chief examiner, J. W. Hayes; assistant examiners, J. C. Bell, W. S. Foster.

Examining unit, Naval Training Base No. 6, Brooklyn, N. Y.: Chief examiner, R. S. Woodworth; assistant examiners, N. J. Melville, G. C. Myers.

Statistical unit: Statistician, E. L. Thorndike; assistants, A. S. Otis, L. L. Thurstone.

The examining of approximately four thousand soldiers in accordance with the plan described above and the comparison of the results with officer's ratings of the men revealed correlation of about .5, and in general justified the belief that the new methods would prove serviceable to the Army.

On July 20, after the adjournment of the Committee on Methods and as a direct result of its work, the following substitute plan for the psychological examining of recruits was forwarded to the Surgeon General of the Army.

Plan for the Psychological Examining of Recruits to Eliminate the Mentally Unfit

This replaces a plan earlier submitted, action on which has been postponed by request.

The committee on methods of the psychology committee of the National Research Council, having completed the preparation of methods especially adapted to the selection of a 'first-line army' and the elimination of bad mental risks, respectfully submits the following plan to the Surgeon General of the Army.

I. *Personnel:* It is proposed,

(1) That six (6) qualified experts, designated hereafter as chief psychological examiners, be recommended immediately for commissions in the Sanitary Corps. One chief examiner shall be located in each camp to organize, direct, and participate in the psychological work. (Pending action on commissions, these men might be employed under the Civil Service Commission as provided in (2) below.)

(2) That eighteen (18) men be employed immediately as assistant psychological examiners under the provision of the Civil Service Commission for the employment of unlisted scientific experts. This will not involve delay, since examination is not required.

Supplementary Memorandum concerning Personnel

If this plan should be acted upon favorably, a list of names for commissions in the Sanitary Corps and for civil service employment, will be submitted promptly upon request.

The committee has a list of 150 men who have signified their willingness to serve the government as psychological examiners or investigators. Of these it is estimated that not more than 75 are both properly qualified and able to undertake this work within a month.

It is further estimated, from information which we have at hand, that within a week 24 men can be made available for service as recommended under sections (1) and (2).

II. *Apparatus and Other Equipment:* It is further proposed,

(1) That there be provided in connection with each camp in which psychological examining is to be conducted:

(*a*) One room for group examinations with floor space approximately 28 by 40 feet, and door at each end. The same to be furnished with 56 tablet-arm chairs (or small tables and chairs), and examiner's table.

(*b*) Not fewer than two rooms with floor space at least 10 by 12 feet, to be used as private examining rooms. Each to be furnished with table and three chairs.

(2) That the following apparatus and supplies be furnished:

(*a*) For each examiner:

(1) Cube construction,
(2) Cube imitation,
(3) Cheap clock,
(4) Stop watch,
(5) Pencils and paper,
(6) Examiner's guide or manual

} Estimated cost, $15

(*b*) For each camp, or examining unit:
(1) Pencil sharpener,
(2) Stock of pencils and paper

} Estimated cost, $10

(*c*) For each recruit examined:
Group record blank, and in certain instances also individual record blanks. Estimated cost, 2 to 4 cents according to number printed.

Memorandum concerning Examining Outfits

Fifteen complete sets of examining materials are at present in use in various army and navy camps and stations. These can promptly be turned over to the government, in case this plan is acted upon favorably.

III. *Procedure in Examination:* It is proposed,

(1) That all recruits be subjected, in groups of about 50, to a general mental examination which shall require not less than 20 nor more than 40 minutes per group. It is the expectation of the committee that this time can be reduced to 20 minutes. If so, the average time per man would be less than ½ minute.

(2) That all recruits, on the results of the group examinations, be tentatively classified as mentally (*a*) low; (*b*) high; (*c*) average; (*d*) irregular, and that as time permits the lowest 10 per cent., the highest 5 per cent., and irregular individuals shall be subjected to more searching individual examination, on the basis of which, report shall be made by the psychological examiner to the proper medical officer.

(3) That all psychological examining be done under authority of the Army Medical Corps, and immediately under the direction of those medical officers who are specially designated to deal with nervous and mental cases.

(4) That the chief purpose of psychological examination be classification of all recruits as first, second or third class

mentally, so that the War Department may use the data of examination as a basis for elimination or for service classification, as policy or necessity dictate.

Copies of the Examiner's Guide, of Form A of the group examination record blank, of the individual examination record blank, and of a special four-page record sheet, are submitted with this plan. These materials have been printed in several hundred copies, and are now being used in an extensive test of the usefulness of the method of examining recruits which is recommended.

Respectfully submitted,

ROBERT M. YERKES,
Chairman, Psychology Committee,
National Research Council

July 20, 1917

Early in August report of the trial of methods of psychological examinations in Army and Navy stations was prepared and on the basis thereof it became possible definitely to recommend to the Medical Department of the Army official trial in the drafted Army of the methods prepared by the committee.

The chairman of the committee was, upon recommendation of Doctors Vaughan and Welch, of the National Research Council, appointed with the rank of major in the Sanitary Corps, National Army, to organize and direct psychological examining for the Medical Department.

It was later decided by the Medical Department to authorize an initial experiment in the following National Army cantonments: Camp Lee, Petersburg, Virginia; Camp Zachary Taylor, Louisville, Kentucky; Camp Dix, Wrightstown, New Jersey and Camp Devens, Ayer, Massachusetts. Arrangements were made for the necessary personnel and equipment, and instructions to commanding generals of the divisions were issued by the War Department.

THE PSYCHOLOGICAL STAFFS OF THE SEVERAL CANTONMENTS

Camp Lee: First Lieutenants Clarence S. Yoakum, George O. Ferguson, Jr., Walter S. Hunter, Edward S. Jones, and

the following civilians: Leo T. Brueckner, Donald G. Paterson, A. S. Edwards, Rudolph Pintner, Benjamin F. Pittenger, Ben D. Wood.

Camp Taylor: First Lieutenants Marion R. Trabue, Karl T. Waugh, Heber B. Cummings, Edgar A. Doll and the following civilians: James W. Bridges, J. Crosby Chapman, John K. Norton, Eugene C. Rowe, J. David Houser, C. P. Stone.

Camp Dix: First Lieutenants Joseph W. Hayes, Harold A. Richmond, Herschel T. Manuel, Carl C. Brigham, and the following civilians: Thomas H. Haines, Norbert J. Melville, H. P. Shumway, Thomas M. Stokes, J. J. B. Morgan, C. C. Stech.

Camp Devens: First Lieutenants William S. Foster, John E. Anderson, Horace B. English, John T. Metcalf, and the following civilians: Raymond H. Wheeler, Harold C. Bingham, Carl R. Brown, Chester E. Kellogg, Ralph S. Roberts and Charles H. Toll.

The work of psychological examining proceeded rapidly, and in December, on the basis of statistical data and reports of inspectors, it was decided by the Medical Department to recommend the extension of psychological examining to the entire Army.

On December 24, 1917, this recommendation was approved by the General Staff and the Medical Department was requested to present a plan for the examining of all newly appointed officers and all drafted and enlisted men. The Section of Psychology, Office of the Surgeon General is now engaged in preparing a plan for the extension of the work.

Official reports of the results of psychological examining will be issued from time to time by the office of the Surgeon General. In this connection it will suffice to quote briefly from the recommendation of the Medical Department.

"The purposes of psychological tests are: (*a*) to aid in segregating the mentally incompetent, (*b*) to classify men according to their mental capacity, (*c*) to *assist* in selecting competent men for responsible positions.

"In the opinion of this office these reports (accompanying

recommendation) indicate very definitely that the desired results have been achieved.

"The success of this work in a large series of observations, some 5,000 officers and 80,000 men, makes it reasonably certain that similar results may be expected if the system be extended to include the entire enlisted and drafted personnel and all newly appointed officers.

"In view of these considerations I recommend that all company officers, all candidates for officers' training camps and all drafted and enlisted men be required to take the prescribed psychological tests."

The work in question has thus far justified itself in the opinion of military authorities on the following practical counts:

(1) As a means of eliminating certain types of mentally incompetent men; (2) as a prompt and convenient method of obtaining for the use of company commanders information which leads to the intelligent assignment and training of men; (3) as a guide to such assignment of men as will result in organizations approximately equal in mental strength; (4) as an aid in the selection of candidates for officers' training groups, for promotion and for special assignment."

The account of the work of this single committee is inordinately lengthy and for the moment it may tend to give an unjust estimate of the significance of other lines of work which have led to important service. It is now proposed to present briefly an account of the development of certain of the more important of these lines of work. They will be considered under the head of committees.

The committee, which under the chairmanship of Dr. Thorndike was charged with problems concerning the selection of men for tasks requiring special skill, has rendered conspicuous service to the committee on methods of examining recruits and the committee on aviational problems. It further has the special distinction of having indirectly furthered the organization of the Committee on Classification of Personnel of the War Department.

The latter organization is due directly to the activities of Dr. Scott who shortly after the special meeting of the council of the American Psychological Association began work in collaboration with his colleagues of the department of psychology, Carnegie Institute of Technology, Pittsburgh, on the preparation of a rating scale for the selection and promotion of officers. Dr. Scott, during July, by personal representation succeeded in introducing this scale in several of the first series of officers' training camps. The trials of the method resulted in favorable and extremely enthusiastic reports from commanding officers to the War Department, consequently the rating scale in revised form was ordered to use in all of the second series of training camps and the Secretary of War, convinced by results already achieved, of the serviceability of psychologists and psychological methods, requested that a special committee be organized to be known as the Committee on Classification of Personnel of the War Department. This committee was organized early in August, 1917.

The need which led to this development is thus briefly stated in Dr. Scott's response to the request of the Secretary of War.

"The attached memorandum makes it apparent that the chiefs of the War Department Bureaus are not wholly satisfied with present methods of classifying personnel and making recommendation for commissions and that they desire to try out the plan outlined below."

"The first aim of this committee is to bring to the aid of the various bureaus the combined judgment and experience of specialists in methods of employing, examining, rating and classifying men according to their native abilities and their relative value to different branches of the service. The second aim is to provide a means for bringing to a focus the combined experience of the various branches of the Army organization in selecting, classifying and assigning men and in recommending them for commission.

"This double purpose requires that the committee consist of (1) a scientific staff of specialists with civilian status,

and (2) a board of coöperating members consisting of a representative from the War College and one officer designated by each chief of a War Department Bureau who chooses to share in the work of this committee. This plan would call for a staff of ten specialists with assistants and a board of coöperating members consisting of ten Army officers."

Psychologists who have served on this committee are Messrs. Angell, Bingham, Dodge, Scott, Shepard, Strong, Terman, Thorndike, Watson and Yerkes. A brief statement concerning the work of the committee prepared by its director, Dr. Scott, on November 12 follows:

My dear Major Yerkes: The work of the committee on classification of personnel in the Army is of such a nature that it is very difficult to state the exact date on which any work begins and is ended. In fact, practically all of our work has developed gradually and so far as we can see now, will never end. It is therefore difficult to give any report.

The following statement, however, will, in a way, conform with your request of November 6:

1. The Rating Scale was made the form for recommending candidates for promotion in eight of the first series of officers' training camps.

2. In the second series of officers' training camps, a permanent record and a rating scale and a pocket rating card were introduced and made a regular part of the system.

3. For selecting candidates for the third series of officers' training camps, the rating scale, a special form of making reports and a pocket rating card are made the official forms.

4. A personnel department has been established in each of the thirty-one divisions now in the United States. Sixteen civilian experts have been employed by the committee to install the personnel department and these sixteen civilians are coöperating with possibly two thousand officers and enlisted men who are devoting their time to this army work. In some of the divisions, this personnel work is developing very rapidly. This is notably true in the case of Camp Upton, where as many as seventy-two officers and three hundred enlisted men have been employed on the work at the same time.

5. The committee has coöperated in the work of psychological examining which is being conducted in four of the cantonments by

the Surgeon General's Department under the direct supervision of Major Yerkes.

6. The committee is coöperating in the work of selecting men for the Signal Corps.

7. The committee is inaugurating a series of trade tests with the hope of making them a regular part of the work in each of the training camps.

8. The committee has formulated a qualification record card for use of officers in the army.

9. The committee has formulated a plan for securing data concerning the eight million registrants who have not as yet been called to the colors.

10. The committee has formulated a plan for promoting, transferring and demoting officers in the army.

11. The committee is formulating a plan to reduce the paper work in the army.

12. The committee has an office in Room 526, War Building, at which place current data are kept concerning all of the men in the National Army and the National Guard. This information is kept on file for the benefit of the Adjutant General of the Army and the Chief of Staff.

13. Seventy-five thousand, six hundred dollars has been appropriated by the War Department for the use of the committee.

WALTER DILL SCOTT,
Director, Committee on Classification of Personnel in the Army

The committee on classification of personnel is not a direct outgrowth of the activities of the psychology committee of the National Research Council, but has resulted from the individual efforts of Doctors Scott and Thorndike who acted as members of an original committee appointed by the council of the American Psychological Association. The work of the committee, as Dr. Scott's summary report indicates, has attained a scope and magnitude which renders adequate account in this connection impracticable. I have endeavored therefore to give such an outline of the work as may serve as a suitable introduction to the more detailed statement which Dr. Scott will make at our meeting tomorrow and so to interest you in this important psychological service that you may all be present to listen to his address.

The original committee on psychological problems of aviation, began the preparation and trial of tests in April under the chairmanship of Dr. Burtt who was assisted by Doctors Miles and Troland. Subsequently Dr. Burtt, because of the need of a vacation, resigned the chairmanship of the committee and Dr. Stratton was appointed in his stead with Dr. Thorndike as executive secretary. Doctors Watson, Maxfield and McComas were added to the membership of the committee.

A large number of measurements on aviational cadets were made in the ground flying schools in Massachusetts, Pennsylvania and California. It is probable that these measurements will ultimately lead to significant practical application.

Meantime, Professor Watson, who in August became a member of the committee on classification of personnel in the Army, accepted appointment as major in the Signal Corps and was placed on duty in the division of personnel. He has been responsible for the organizing of methods, other than the medical, of the aviational examining boards, and has further been charged with the direction of the work of a group of research psychologists.

His own summary statement concerning this work, which pertains to the effects of high altitude, is presented herewith:

> The work of the Oxygen Committee is being carried out under the general supervision of Major Lewis, Major Wilmer, Major Watson and Captain Yandel Henderson.
>
> Preliminary investigations are being undertaken at the Bureau of Mines, American University, Washington. Several men in physiology, psychology, and general medicine have been commissioned and are undertaking the actual experimentation. Captain Dunlap is in immediate charge of the laboratory work in psychology. Captain Schneider is in immediate charge of the work in physiology. On January 1, the work will be continued at Mineola, Long Island, where a small laboratory has been built. At present, the work is being carried out upon re-breathing apparatus. A steel tank which can be exhausted will be used in later experiments. In this way, the factor of barometric pressure can be controlled and the aërial conditions more closely simulated.

Psychological members at present on aviational examining boards are: Messrs. Wells, Bentley, Hamilton, Stratton and Henmon.

The work of the aviational committee and of Major Watson, its military representative, is clearly of great importance but it is utterly impossible in this brief address to report results or even to indicate satisfactorily the directions in which psychologists are contributing and may hope to contribute to the success of aerial warfare.

The committee on psychological literature relating to military affairs consists of the chairman only, Dr. Bentley, who for weeks sought out pertinent publications and forwarded to the sub-committees references and summaries of articles. In this way substantial aid was rendered psychologists who were engaged in the development of new methods and in the attempt to make practical application of existing methods of psychological research to special military problems.

The committees on the psychological problems of incapacity, including those of shell-shock, reëducation, etc., under the chairmanship of Dr. Franz, and on the psychological problems of vocational characteristics and vocational advice under the chairmanship of Dr. Watson were subsequently combined under the chairmanship of Dr. Franz who with the assistance of Doctors Watson and Lashley has furthered the development of reëducational methods and has attempted to coöperate in various ways with the appropriate committees of the Council of Defense and with the Division of Special Hospitals, Office of the Surgeon General.

This field of work is obviously of extreme importance and will doubtless demand the services of many psychologists, among whom women may be numbered, as the war continues and incapacitated men are returned to this country.

Dr. Coe, chairman of the committee on psychological problems of recreation in the Army and Navy, promptly organized his committee and instituted investigations which should lead to the establishment of proper coöperative relations between his group and the various agencies actively

concerned with recreational activities in military stations. Dr. Coe's latest report indicates that his committee is attempting so to formulate psychological problems of military recreation as to decide wisely on lines of work which should be prosecuted immediately for the assistance of the recreational organizations.

Pedagogical and psychological problems of military training and discipline, in the interest of which Dr. Judd was named as chairman of the committee, received no attention because of the chairman's inability to find time for additional work. Dr. Judd finally resigned the chairmanship and the Psychology Committee of the National Research Council elected Dr. Bagley in his place. As yet nore port concerning plans for the organization of the committee or for the prosecution of possible work have been received from Dr. Bagley. It is believed that this committee should be able to render signal service to the War Department by carefully analyzing various forms of military training and discipline and by suggesting new methods or modifications of old methods in the light of pedagogical and psychological knowledge.

The chairman of the committee on problems of motivation of military service, Dr. Scott, has been so far occupied with work on his rating scale and with his duties as director of the committee on classification of personnel that he has been unable to give special attention to the tasks outlined for his original committee. It is hoped that in the future opportunity may be found for special studies of motivation.

Similarly the committee on problems of emotional stability, fear and self-control, whose chairman is Dr. Woodworth, has thus far been unable to find suitable opportunity for practical work. According to the latest statement received from the chairman an attempt is being made to develop methods which shall aid in the elimination of men who are emotionally unstable.

Dr. Seashore's committee on acoustic problems of military importance has recently been invited to advise concerning methods suitable for military use and the chairman has submitted suggestions which promise to be of practical importance.

One of the first psychological problems suggested to the psychology committee of the National Research Council for solution was referred in May to Dr. Dodge, chairman of the committee on visual problems of military significance. This had to do with the needs of the Navy and its prompt and successful solution by Dr. Dodge led to increasingly important demands for his professional services. Much of the work which this committee has furthered is strictly secret. I therefore shall make use of a statement concerning the work of his committee which Dr. Dodge has prepared for me. Otherwise, I might make the mistake of telling something which I do not know!

Navy activities of the Psychology Committee: The Psychology Committee has been of service to various enterprises, two of which, involving more than one of our subcommittees, may not be mentioned at this time.

In addition to these the committee on personnel has prepared a qualification record adapted from the qualifications record of the army to the particular requirements of naval service. This has not yet been officially adopted, but preliminary trials have been ordered to ascertain its defects in operation, if there are any.

For the psychology committee of the National Research Council Dr. Dodge undertook the analysis of the reactions and coördinations which are involved in gun-pointing, for the purpose of devising tests to discover special fitness for the several tasks with a minimum loss of time. From this analysis a set of tests for the fire control party was elaborated with the coöperation of interested naval officers. This is now in practical use in the Atlantic Fleet.

Also with the coöperation of various naval officers he devised a graphic instrument to record the pointing reactions, the accuracy of coördinations in keeping on a moving target, and the accuracy of fire. This instrument was put through a severe trial on two of the major fighting units of the navy. It proved its value and has been elaborated into a robust and fool-proof land training instrument. This has already proved highly satisfactory in one of the training stations and has been ordered reproduced for general use.

Recently the psychology committee of the National Research Council has appointed two additional subcommittees, the functions of which are of such nature that they may not

be discussed without lessening the prospective value of the work of the committees.

The obvious and significant trend of our psychological military work is toward service. Psychologists who develop methods or accumulate information which promises to aid us in winning the war are shortly appointed to positions which give them opportunity to apply their special knowledge effectively. At present our profession is importantly represented in the Department of the Adjutant General, in the Navy, the Signal Corps, the Medical Corps, the Sanitary Corps and the Quartermaster Corps. In some of these bureaus men have been commissioned. In others, psychologists are serving under civil appointment.

As we look ahead and attempt to prophesy future needs in the light of occurrences of the past six months, it is clear that the demand for psychologists and psychological service promises, or threatens, to be overwhelmingly great. In the Sanitary Corps alone, for the conduct of psychological examining, it is safe to assume that at least one hundred and fifty men will be needed for military appointments during 1918. For reëducational work during the same period it is probable that at least one hundred men or women, trained in the use of psychological and educational methods, will be required by special hospitals. In the aviational section of the Signal Corps the chances are that all psychologists properly qualified by scientific training who are available can be utilized to advantage. The Navy makes no personnel demands but it is using to very great advantage the professional advice of psychologists properly qualified to give such advice. The committee on classification of personnel has needs which cannot possibly be met by the available supply of well-trained psychologists. It behooves us therefore to maintain, and if possible improve, the facilities for psychological training in our educational institutions and the quality of instructional work. There should be no attempts at short cuts, but instead capable students should be encouraged to train themselves thoroughly for military positions which demand professional competence in addition to sound common-sense, good judgment and strong personalities.

Our profession has brought to the front the desirability and the possibility of dealing scientifically and effectively with the principal human factors in military organization and activity. It is by no means improbable that the work which has been initiated by the council of the American Psychological Association, by the psychology committee of the National Research Council and by the committee on classification of personnel will ultimately lead to the organization of a new bureau or corps within the military department. This bureau would be responsible for the varied human problems which experimental psychology has revealed to the military authorities and has dealt with in effectively practical ways.

It has been my desire in this historical account of our psychological services to give personal credit so far as possible. I cannot close without making grateful acknowledgment to those who have prepared the way for various lines of psychological work and materially furthered our progress. The list is necessarily incomplete. The authorities of the Military Hospitals' Commission of Canada rendered substantial aid at the outset by revealing needs of psychological assistance. Dr. George E. Hale, chairman of the National Research Council, opened the way to service by recognizing our science and by giving it a place in the Council of Defense. Major Victor C. Vaughan and Major William H. Welch foresaw the possibilities of value in psychological examining and took the risk of recommending psychological service to the Medical Department of the Army. Major Edgar King and Major Pearce Bailey materially assisted in the formulation of plans which made possible the proper coördination of psychological work with military training. Colonel Henry A. Shaw, through his thorough inspection of psychological examining and his personal interest in the extension of psychological methods to the entire Army, hastened the latter achievement and greatly encouraged the psychological staff. Surgeon General Gorgas and his staff hospitably received us and supplied ample opportunities for service.

Finally, it remains for me to say that from the very start

there has been conspicuous enthusiasm for psychological military service and loyalty in the service. The work has been arduous, the discouragements often numerous and serious but in spite of them our various lines of work have been carried forward satisfactorily and in most instances with surprising rapidity. Everyone who has had opportunity to share in the work obviously feels that he has contributed to our military progress and has rendered more substantial service through the application of his professional training than would have been possible in any other line.

AN EXPERIMENT IN EMPLOYMENT PSYCHOLOGY

BY HENRY C. LINK, PH.D.

New Haven, Conn.

The experiment described below was carried on during March and April, 1916. It is the first of an extensive series of such experiments, which is being conducted by the Winchester Repeating Arms Company.

Two types of work were chosen as the field for this experiment. The first was that of shell inspection, the second that of gauging shells for minimum and maximum head-thickness. These two operations were carried on in two long, well-lighted rooms, by about 330 girls. About two thirds of the girls were engaged in inspecting, and one third in gauging. This large number of girls offered the opportunity of making a sufficiently extensive series of observations. The further fact that nearly all of these girls were doing piece work, and that most of them were working on the same kind of shell, made this particular field an ideal one as far as furnishing an objective basis for correlations was concerned. The presence of 'scrap' inspection added still further to the reliability of this objective basis.

The general purpose of the experiment was to discover a set of tests which would guide the employment section in selecting candidates for these two types of work. The more specific purpose at this stage was to discover a set of tests which would show a correlation with the 'output' or 'production' of the girls. If it could be demonstrated that the fastest workers were consistently fastest in certain of the tests and the reverse, then it would be reasonable to assume, subject to further proof or disproof, that these tests could be used by the employment section as a basis for selecting applicants. Moreover, it was also necessary to discover what

standards to set even in those tests which showed a correlation.

The first step, after the field had been decided upon, was the choice of materials or tests. The ideal method here would have been the method of miscellaneous empiricism, but the practical exigencies of the situation made an arbitrary choice of tests necessary. In order to make this arbitrary choice as intelligent as possible, it was necessary first of all to analyze the work of the girls to be tested into its fundamental elements. The work of shell inspecting, for example, was done at a table like an upturned shallow box. Upon this hollow table was dumped a large box of brass shells, not yet loaded, and all of exactly the same kind. The work of each girl was to inspect these shells and throw out those that were defective. In doing this, a girl would first gather up a large handful of shells, as many as could be piled in one hand, being careful to have all of them pointing in the same direction. Then she would put both hands around the shells and turn them all up so as to expose their insides. She would then look down into every shell for dents, scratches, stains, and other very minute defects. When any such defect was discovered, the shell was skillfully extracted from the pile and thrown into one of three or four appropriate scrap boxes. The entire handful was then turned over, and the head of every shell examined for various defects. The shells were then held in a horizontal position on the left hand, and allowed to roll from the pile into the right hand. Each shell in the process of rolling from one hand into the other, exposed its lateral surface and was closely scrutinized for scratches, oil-dents, stains and other defects. The good ones were taken by the right hand and dropped into a pocket at the left side of the table through which they fell into a box below.

An analysis of this operation showed that it required the following qualifications:

1. Good eyesight. The defects to be detected were often so minute as to be indistinguishable to any but the best of eyes. It took the experimenter almost a minute to see one of the most common defects which these girls were required

to notice in an instant. Any weakness of the eyes or marked difference between the two, would be likely to show bad results in the inspection.

2. Keen visual discrimination. Good eyesight is not sufficient. The inspector, looking at a whole handful of shells must, with a few glances, be able to recognize those which are defective and remain oblivious to those which are not.

3. Quick reaction, that is, the ability to extract, as quickly as seen, the defective shell and toss it into the appropriate box.

4. Accuracy of movement, required in picking out the right shell from the closely held handful. This requires a very peculiar kind of deftness, and, in order to facilitate it, many of the girls allow their finger nails to grow to an unusually long and sharp point.

5. Steadiness of attention. The least wavering of the eyes or letting up of the attention is likely to allow some bad shells to slip by or to lengthen the operation.

The above analysis having been made, the next step was to find tests which would be likely to bring out the presence of these qualities. In this connection I wish to acknowledge my indebtedness to Professor H. L. Hollingworth, through whose suggestions concerning the choice of tests the results of this experiment are in a large measure due. In choosing the tests the experimenter had constantly in mind several requirements which their ultimate use in the employment section would make necessary.

1. They would have to be short. The more complex tests and the intricate apparatus used in the psychology laboratory would require too much time for use in an employment bureau.

2. They would have to be easily understood by the person who was taking them, and would therefore have to be of a very simple nature. Obviously, if the tests were to require a considerable amount of instruction and a long period of learning before they showed satisfactory results, they could not be used by an employment bureau. It would be just as well in that case to put the applicant at work and see whether she could learn to do the job.

3. The tests would have to be of fundamental significance. It would not be practicable to devise a set of tests peculiarly adapted to only one type of work, because in that case it would be necessary to have as many sets of tests as there were types of work. Therefore, the tests should be of such nature as to be capable of bringing out basic qualities; not only those involved in the work of inspection but such as would indicate adaptability in other types of work apparently different, but really involving the same fundamental qualities.

4. The tests would have to be of such a nature as to avoid the difficulties arising from the lack of elementary education. This applies only to the use of tests where a school education in English is a non-essential. For instance, two of the best inspectors in the room did very poor work in those tests which required familiarity with English letters. It turned out that these girls were Italian and had only been in this country for a short time. One could hardly speak English and the other had gone to an English school for only three months. It appeared necessary, therefore, to use tests in which this difficulty would be reduced to a minimum.

With these conditions in mind, sixteen different tests were chosen and prepared for preliminary trials. It is not possible or necessary to go into detail on all these tests at this point. However, each test was tried out on a large enough number of girls to give a good indication as to whether it was likely to prove significant. A good deal of time was required for this part of the experiment, but eventually a set of eight tests was selected for the body of the experiment.

An important problem to be settled was the question as to just how these tests should be given. The rooms in which the girls were at work were very noisy due to machines and the handling of thousands of brass shells. Should the girls be tested in this noisy atmosphere or should they be taken off to some quiet place, free from any possible disturbance? In the end, it was decided to give the tests in the work room, on the supposition that if the subjects were left in their regular environment, they would be more likely to show characteristic results in their performance in the tests. A small room

was screened off on one side and this served as a place in which the tests could be given with comparative freedom from intrusion.

Some difficulty had been anticipated in putting the tests into operation, both on the part of the girls to be tested and because of the confusion that might be introduced into the running of the shop. It was feared that the girls might resent the experiment as an infringement upon their personal liberties. However, through perfect frankness in explaining to the girls the exact purpose of the tests, and through the help of the foreman and instructors in removing the air of mystery and suspicion which naturally would surround such an experiment, and through the use of a 'matter of fact' procedure which took each girl's acquiescence for granted but still refrained from the slightest indication of compulsion, it was possible to carry out the entire series of tests without a single unpleasant occurrence. At the outset, an instructor took the girl to be tested from her work and brought her into the room and remained there while the tests were being given; but after a short time, even this precaution was unnecessary. The experimenter became a fixture in the shop and could, without the least embarrassment to the girl, bring her into the experimental room for the tests. Thus, a great deal of time and trouble was spared to the foreman and his assistants.

The eight tests chosen for the body of the experiment were as follows:

1. A simple eyesight test with the use of the Lowell chart.

2. A card sorting test. The subject was given a pack of 49 cards, upon the face of each one of which from 7 to 12 letters were distributed promiscuously. Twenty of the cards contained the letter 'O' and the rest did not. The subject was asked to sort these cards into two piles, those which had 'O' on them and those which did not. The time required for this performance was taken and the number of errors recorded. The object of this test was to bring out the subject's ability to pick out the essential element from a more or less heterogeneous collection of elements, and also, in some measure, to bring out the deftness of the subject in

handling the cards. These cards were so marked and numbered on their reverse side that, after every test, it was possible for the experimenter to sort them back into their original order and to observe the number of mistakes that had been made. In this way it was made possible for every subject to perform this test in exactly the same manner.

3. A modification of the Whipple accuracy test. The subject was required to take a brass-pointed pencil and stick it successively into a series of nine graduated round holes in a brass plate. The speed of the subject's movements was controlled by a metronome set so as to allow thirty trials per minute. The brass-pointed pencil was wired in circuit with the brass plate containing the holes so that, whenever the brass point touched the side of the hole or any part of the brass plate, the circuit was closed; and as soon as the circuit was closed, it produced a click in a telephone receiver which the subject held to her ear. At the start of the test, the subject was instructed to put the brass pencil into each hole in succession until she heard a click in her ear, when she was to start all over again. The experimenter also had a telephone receiver to enable him to follow the subject's performance. The holes were numbered 1, 2, 3, etc., to 9. As soon as the subject failed to put the pointer squarely into a hole but touched the brass plate and produced a click, the experimenter recorded the number of the hole at which she had failed. This constituted one trial. Each girl was allowed fifteen such trials, and the numbers of the last ten were taken and averaged, the first five serving as preliminary practice. For instance, if a girl, in her first trial, reached the fourth hole and missed on the fifth, the number five was recorded; if she missed on the sixth, six was recorded, etc., until fifteen numbers had been taken. Then, the sum of the last ten trials divided by ten, gave the average performance for the subject. The larger the average, the better the performance. This test occupied from two to three minutes.

4. The Whipple steadiness test, also modified. The apparatus for the accuracy test and steadiness test was set up as a single piece and wired so as to enable the subject

and observer to detect the contact by means of telephone receivers. This method was found to be much more delicate and reliable than the buzzer method. In each case, fifteen trials were taken. The points reached before contact in the last ten trials were averaged and constituted the record for the test.

5. The Woodworth-Wells cancellation test. The subject was requested to cross out, with a pencil, every '7.'

6. The Woodworth-Wells 'Easy Directions' test.

7. The Woodworth-Wells number checking test, in which the subject was asked to place a check opposite every group which contained both a '7' and a '1.'

8. A modification of the tapping test, in which the subject was requested to push down, as rapidly as possible, a telegraph key to which was attached a Veeder counter. The number of recorded thrusts over a period of one minute constituted the record for that performance.

These eight tests, including the time required to take a girl from her work and back to it, occupied a period of not more than twenty-five minutes.

However, before the tests were given, the subject was questioned as to her age, the length of time she had been at her present work, her previous work, how long she had worked on day rates before being given piece work, also the extent of her education and the nationality of her parents. The employment section had already furnished cards containing a large part of this information, and so it was only necessary to supplement what was already at hand. The questions, however, also served the very useful purpose of decreasing the initial nervousness of the subject, and thus prepared the way for the tests proper.

Seventy-three girls were tested, 52 of whom were inspectors, and twenty-one gaugers. The work of gauging will be described later. It was impossible to test a larger number of girls because the experiment came at a time when the work of shell inspection was rapidly slowing up and a majority of the girls were laid off or transferred to another job.

In determining the results of the tests, the first step was

to obtain the ranking of the girls as shown by their daily work. The experimenter had, while conducting the tests, also kept a record of the number of pounds of shells inspected by each girl on the day that she was tested. However, this record was not deemed extensive enough to afford a reliable criterion of a girl's productiveness. To be sure, if a girl's work on the day that she was taking the tests was unusually high, that fact might show up in an unusually good performance in the tests, and thus serve to maintain the correspondence between the two. However, the object of the test was such as to make an immediate correspondence a distinctly minor feature. It was rather to discover whether any correspondence existed between the performance in certain tests given for the first time and occupying only a few minutes, and the work of a girl over an extended period of weeks and even months. Unless such a correspondence could be shown, the tests would be of little worth to the employment section. Therefore, it was decided to take as the basis of the girls' standing, an average of their work for four weeks. These averages were obtained by making a detailed statement, drawn from the separate daily production slips of each girl, showing the number of pounds done for every day in the week, together with the exact number of hours taken to do them. The total number of pounds inspected by each girl divided by the total number of hours worked gave the average number of pounds per hour for the particular girl and became the basis for her ranking. In making out these statements, all work other than that on a single kind of shell was discarded. This was done for the sake of uniformity, it being manifestly unfair to judge the relative speed of different girls on a basis of pounds when one girl was inspecting large shells which went very fast while another was inspecting small shells which went very slowly.

After the average hourly production for a period of four weeks had been determined for each girl, the performances in each one of the tests were compared with the production rates to determine the correlation which existed between them. This was done by the method of rank-differences using the formula

$$R = 1 - \frac{6\Sigma D^2}{n(n^2 - 1)}.$$

The Pearson product-moments method was used to check the results obtained in the case of the three tests which showed the highest correlation. The difference in the results was negligible. The tests themselves were treated on the basis of speed only, corrections for mistakes being made in terms of time. The Correlations found were as follows:

	C. of C.	P. E.
Card-sorting test	.555	.069
Accuracy test	.377	.085
Steadiness test	.238	.092
Tapping test	.135	.096
Cancellation test	.635	.059
Easy directions	.141	.093
Number-group checking test	.734	.045

Eye test: Because of the nature of the test, only individual correspondences could be shown. Some of these will be mentioned later. It was plainly evident, however, that an inspector required two very good eyes in order to succeed at this work.

From the above figures it will be seen that all the correlations in the case of these eight selected tests are positive, and that three or four of them are quite high.

On the other hand, when the same tests were compared with the work of twenty-one gaugers in the same thorough way, the results were quite different. We may well attribute this difference to the difference in the work. The work of the gauger does not require the use of the eyes to any extent. The operator simply picks up a handful of shells and, with or without looking, tries the head of each shell on a gauge containing two openings. The shells which are too small pass through the first opening into a reject box below. Those that are too large fail to pass through the second opening and are thrown into another box at one side. The operators sit in front of their gauge, which is rigidly fixed, and try each shell at one opening and then at the other, just as rapidly as they can move their hands up and down. It will readily be seen that this work requires capacities quite different from

those needed in the work of inspecting. This difference is fairly well indicated by the differences in the coefficients of correlation found for the two types of work and placed side by side below:

	Inspectors		Gaugers	
	C. of C.	P. E.	C. of C.	P. E.
Card-sorting test	.555	.069	.053	
Accuracy test	.377	.085	.213	
Steadiness test	.238	.092	.258	
Tapping test	.135	.096	.516	.071
Cancellation test	.635	.059	.167	
Easy directions	.141	.093	.183	
Number-group checking test	.734	.045	−.187	

It so happens that the very test which shows the highest correlation among inspectors shows the lowest correlation, a minus correlation in fact, among the gaugers. This is quite in accord with the apparent fact that for the work of inspection, visual discrimination is probably the most important qualification, while for the work of gauging it is probably the least necessary. The only test which shows a significant correlation among gaugers is the tapping test. This seems reasonable since in both the test and the operation of gauging, speed of movement and endurance are the chief factors. The significance of the other tests is chiefly negative since they serve to bring out the fact that girls who to the ordinary observer and even to the trained employment manager look very much alike, may still possess very different sets of qualifications. If all the gaugers and inspectors had been lined up before the employment window, it is highly improbable that the employment manager would have been able by mere observation to make the radical distinction between the applicants which the tests would have enabled him to make.

In order further to prove or disprove the results found in the first section of this experiment, the three tests which were found to show the highest correlation for inspectors were given to twenty-eight cartridge inspectors, ten bullet inspectors and 30 paper shot shell inspectors. A summary of all the correlations for these three tests is given below.

Kind of Work	No. Girls	Card Sort. Test		Canc. Test		No. Group Checking	
		C. of C.	P. E.	C. of C.	P. E.	C. of C.	P. E.
Shell inspecting	51	.55	.07	.63	.06	.72	.04
Bullet inspecting	28	.49	.10	.26	.13	.58	.10
Cartridge inspecting	10	.52	.16	.48	.17	.62	.16
P. S. S. inspecting	30	.13	.12	.11	.13	.02	.13

From these figures it can be seen that the correlations for the cartridge and bullet inspectors come quite close to those found for the shell inspectors but are not quite so high. However, when these tests were being given to the cartridge and bullet inspectors about four fifths of the girls had been laid off and those who remained were, on the whole, girls whose speed was in about the same class. The large individual differences, therefore, which appeared in the work of the shell inspectors were not present and this may account for the slightly lower correlations found on these jobs. The average individual difference or the A.D. from the average hourly production for each of the entire groups was as follows:

	Per Cent.
1. Shell inspectors	27.7
2. Bullet inspectors	18.7
3. Cartridge inspectors	6.9
4. P. S. S. inspectors	2.2

It will be seen from these figures that there is a direct relation between the degree of correlation for these tests and the amount of average deviation for each of the four classes of workers. Through the reduction of the working force, the extremes were eliminated at both ends—the slow girls being discharged, and the very fast ones modifying their pace to suit the retarded pace of production. Therefore, although the individual differences still appeared in the tests, their absence in production rates made it impossible to obtain as fair a comparison between the performance of the girls in the tests and their actual ability as workers as was obtained in the first part of the experiment.

However, the results in their entirety were such as to justify the use of the three most significant tests in the employment section. The employment section is giving these

tests to all applicants for the work of inspecting described above. So far, 78 girls have been chosen on the basis of these tests. The results of these choices are being carefully watched. As soon as production figures for the new girls are available, the tests given will again be correlated with the average hourly output of the girls. In this way it will be possible to discover whether the correlations previously found continue to exist under the new conditions imposed by the mechanism of employment.

ASSOCIATIVE AIDS: I. THEIR RELATION TO LEARNING, RETENTION, AND OTHER ASSOCIATIONS

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The particular problems of this experiment center about (1) the relation of associative aids to the rate of learning and of forgetting; (2) their relation to other associations; (3) their relation to the transfer of learning; (4) their relation to repeated learning or practice; (5) their relation to thinking and the theory of thought; and (6) their relation to methodology in psychology.

The material used in this experiment consisted of the following pairs of words or syllables:

SERIES *SB*

1. sauce	balloon
2. mistake	clean
3. elephant	steeple
4. simmer	tarry
5. ring	kitten
6. turkey	among
7. chest	muffin
8. galley	time
9. kimono	stencil
10. space	betide

SERIES *RC*

1. radish	coffee
2. serve	hold
3. pork	cocoa
4. cheese	tomato
5. speak	weigh
6. ribbon	banana

7. being	credit
8. celery	wafer
9. troll	blast
10. soup	wall

SERIES *WS*

1. Wipfel	summit
2. beharren	preserve
3. Korper	body
4. anregen	stimulate
5. Dach	roof
6. ereignen	happen
7. Angriff	attack
8. besonder	particular
9. Citrone	lemon
10. geigen	fiddle

SERIES *WP*

1. wum	pir
2. gab	lek
3. vif	gom
4. mif	jas
5. heb	tup
6. beq	nog
7. zis	ruc
8. paf	zal
9. dev	cux
10. lir	wap

The experimenter took one series at a time, read it once to the subject, and then following the prompting method, required the subject to respond with the second word of each pair as soon as possible after he gave the first word. There were 27 subjects. With the exception of nine, each subject was asked after he had learned the series to report any associations that he had formed between a given pair. The subjects of the exception were asked after every response to tell of what they had thought between the stimulus and the response.

Before beginning the first series, the subject was instructed as follows: "I am going to read some pairs of disconnected words to you like book-ditch or feel-reply. I want you to repeat them after me the first time I read them. The second time I am going to say only the first word of each pair, and I want you to name the second word. If you don't remember it, just say 'No,' and I will prompt you. After you have learned the words, I am going to ask you to tell me whether or not you thought of anything by which you connected the pairs. For example, with book-ditch you might think of a book lying in a ditch, or just a red book, or you might have thought of nothing in particular. In that case, you just tell me that you didn't think of anything." The experimenter gave the stimulus words in the order given above except when a word was correctly responded to in a test. In that case, he skipped that word the next time he went through the series. He kept a record of every wrong reaction, of every reaction time as it was measured by a stop watch, of the number of promptings for each pair, and of every association.

The subject's retention was tested for the four series in the manner described every day for six successive days. On the sixth day three additional tests were given, the B, D, and U tests. In the B test, the subject had to learn one or two series backwards, *i. e.*, name the first word of a pair after the experimenter gave the second. In the D test, the subject had to learn the first word of a series downwards, *i. e.*, name the first word of a pair after the first word of the pair given by the experimenter. The U test was just the reverse of the D test, *e. g.*, when the experimenter gave the first word of the last pair, the subject had to name the first word of the pair just before it. Verbal reports of these tests were required just as in the regular tests.

I

This paper will be limited to the first two problems stated above. Since the relation of associative aids to the rate of learning and of forgetting cannot be conveniently discussed

separately, we shall discuss both of them together. By an associative aid, I understand some association that either ties the words of a pair together for the learner or ties one of them to some part of his experience. In the example given above, the thought of a 'book' lying in a ditch ties the words book and ditch together, and the thought of a red book ties the word 'book' to a part of the learner's experience.

To discover the relation of associative aids to the rate of learning and of forgetting, we might first discover how these two factors are related in themselves and afterwards hunt for a connection between this relationship and the associative aids. The relation between the rates of learning and forgetting may be discovered in three ways: (1) by seriating the average learning times, L.T.'s, of the pairs of each series for the first day of the different subjects in their order of quickness of learning and then finding the relation of the averages of the four quarters to the average L.T.'s of the corresponding quarters twenty-four hours later; (2) by setting up arbitrary limits for quickly and slowly learned pairs and then finding the relation of the L.T.'s of these pairs to the corresponding L.T.'s twenty-four hours later; (3) by setting up arbitrary limits for quickly and slowly forgotten pairs, and then finding the relation of the L.T.'s of these pairs to the L.T.'s of the corresponding pairs twenty-four hours earlier. I have followed the three methods to some extent but have made my principal calculations according to the third method.

In Table I. are presented the results according to the first method. The second and third quarters are averaged into the middle half. It will be seen that if retention is measured by the Saving Method, the old saying 'Quickly learned, quickly forgotten' is the conclusion.

The amount forgotten is in direct proportion to the quickness of learning. However, we may measure retention by the absolute length of the learning time, and in this case the amount forgotten varies inversely as the quickness of learning. To determine the relation between the rate of learning and the rate of forgetting we must first decide which is the better method of measuring retention.

TABLE I

	S.B.					R.C.				
	First Day	Second Day	Per Cent. S.	N.	N.S.	First Day	Second Day	Per Cent. S.	N.	N.S.
Ave. L.T. of first quarter.....	3.07	2.73	11	60	6	2.45	2.28	7	50	5
Ave. L.T. of mid. half.....	6.57	4.18	36	130	13	4.52	3.48	23	120	12
Ave. L.T. of fourth quarter....	14.16	6.59	53	60	6	10.19	6.21	39	50	5

	W.S.					W.P.				
	First Day	Second Day	Per Cent. S.	N.	N.S.	First Day	Second Day	Per Cent. S.	N.	N.S.
Ave. L.T. of first quarter.....	5.49	3.40	38	70	7	8.96	4.97	44	70	7
Ave. L.T. of mid. half.....	10.44	5.85	44	130	13	15.59	7.41	52	130	13
Ave. L.T. of fourth quarter....	19.02	10.12	46	70	7	31.15	12.21	61	70	7

The L.T.'s are in seconds; Per cent. S. = Per cent. saved; N. = No. of pairs; N.S. = No. of subjects.

The Saving Method furnishes a percentile measure of the amount of repetitions or the time saved in learning, but this does not necessarily correspond with the amount retained. Let us say that *A* learns a stanza of poetry in 2 minutes and relearns it in 1 minute. *B* learns the same stanza in 10 minutes and relearns it in 3 minutes. *A* saved 50 per cent. and *B* 70 per cent. *A* will reproduce three times as much in the same unit of time as *B* and the entire passage in ⅓ of *B*'s time. Shall we then say that he forgot more than *B* because he saved 20 per cent. less of his original learning time? If *A* forgot more than *B*, how could he reproduce three times as much in the same unit of time? If we wish to measure the efficiency of retention, it is evident that we must limit ourselves to the amount reproduced per unit of time. The same rule applies if we wish to compare the efficiency of one subject's retention with another's. In both of these cases, the Saving Method is misleading, and in every case where the learning times are not constant. Its value depends upon the amount of difference between the learning and relearning times in relation to the learning time. This value according to Table I. varies directly with the quickness of learning and relearning, and therefore inversely as the

amount reproduced per unit of time in the retention test. It appears, therefore, not to be a good measure of retention. If so, the facts of Table I. mean that the rate of forgetting varies inversely as the rate of learning. But if it is doubtful what we mean by 'forgetting' or 'retention,' we can at least say that the rate of relearning varies directly with the rate of learning. From the standpoint of efficiency both statements mean the same thing, and, for our purpose, that is sufficient.

The effect of associative aids upon the relation of the rate of learning and of forgetting could be ascertained in part by finding the per cent. of the pairs for the subjects in the first quarter, middle half, and fourth quarter, respectively, that were learned by the aid of association. I have not made this calculation because of the composition of these averages which distribute the individuals only with respect to their rates of learning. But each individual has some pairs which were much more slowly learned than others, some pairs that were learned associatively, and some pairs that were learned mechanically, *i. e.*, without association. The mixture of these quantitative and qualitative elements in one average destroys the true quantitative relationship between the rate of learning and of forgetting and covers up the effects of the qualitative modes of learning, *i. e.*, of the associative aids. The effect of the latter is much more readily discovered if we first sort out the quickly and slowly learned pairs, as in the second method, or the quickly and slowly forgotten pairs, as in the third method.

The second and third methods are probably of equal value, but I have principally followed the third method since it was very easy to set up an arbitrary limit between quick and slow forgetting. I called those pairs slowly forgotten, S.F., which were responded to correctly in the second day without prompting, and those that required one or more promptings I called quickly forgotten, Q.F. I then averaged the L.T.'s of the first and second day for these two sorts of pairs for each subject, then seriated the subjects in their order of quickness of learning for the first day for each sort of pairs,

and found the averages for the first quarter, the middle half, and the fourth quarter, respectively, as in the first method. The results for the four series appear in Table II., which also contains the percentile values of retention, the number of pairs, and the number of subjects for each quarter or half.

Table II. shows that the S.F. pairs, those responded to correctly on the second day without prompting, were quickly learned on the first day, *i. e.*, that quick relearning is accompanied by quick learning. This is evident from a comparison of the L.T.'s of the Q.F. and S.F. pairs, quarter for quarter and half for half for each series. With a few exceptions, we can again say that the rate of relearning varies directly as the rate of learning, also that retention as measured by the Saving Method varies directly as the length of the learning time for both Q.F. and S.F. pairs, and for each kind of material. But the striking fact is that quarter for quarter and half for half the retention for the S.F. pairs is invariably higher than for the Q.F. pairs. Whether we measure retention by the Saving Method or by the length of the learning time, it is higher in every case for the quickly learned than for the slowly learned pairs. The old saying 'Quickly learned, quickly forgotten' must therefore be changed into 'Quickly learned, slowly forgotten.' This relationship is also affected by the quality of the material. The often repeated statement that meaningful material is forgotten more slowly than nonsense material must be qualified in several ways: First, with respect to the Q.F. pairs, the more meaningful material, the more rapid the forgetting; second, with respect to the S.F. pairs, the more meaningful the material, the slower the forgetting. These statements are based on the average per cent. S. for the Q.F. and S.F. pairs of each series.

We are now in position to state the relation of associative aids to the rate of learning and forgetting. This was determined by calculating the number and the per cent. of Q.F. pairs and of S.F. pairs that were learned with an association on the first and second days, respectively. The results are given in Table III. In this table, we notice first the large differences between the Q.F. and S.F. pairs that were learned

TABLE II

	S'B.										R.C.									
	Q.F.					S.F.					Q.F.					S.F.				
	First Day	Second Day	Per Cent. S.	N.	N.S.	First Day	Second Day	Per Cent. S.	N.	N.S.	First Day	Second Day	Per Cent. S.	N.	N. S.	First Day	Second Day	Per Cent. S	N.	N.S.
Ave L.T. of first quarter....	3.40	6.23	−81	26	6	2.06	2.43	−13	37	6	2.05	5.01	−144	11	5	2.27	2.18	4	31	5
Ave L.T. of mid. half.....	8.47	14.07	−66	48	11	4.23	2.20	48	71	11	5.58	5.64	−1	42	11	4.31	2.22	48	82	11
Ave L.T. of fourth quarter..	15.03	8.17	45	27	6	9.47	2.37	72	41	6	14.56	5.76	60	18	5	8.84	3.32	62	36	5
Ave. L.T. of all.....	8.86	7.43	16	101	23	4.99	2.30	54	141	23	6.94	5.52	20	71	21	4.90	2.38	51	149	21
A.D.....	3.72	2.50				2.22	.91				2.17	2.29				2.11	.72			
	W.S.										W.P.									
Ave. L.T. of first quarter...	6.01	5.38	10	35	7	1.78	1.67	6	34	7	9.54	6.00	37	46	7	4.93	1.79	63	16	6
Ave. L.T. of mid. half.....	14.06	10.75	23	62	13	4.83	1.79	63	56	13	17.06	9.90	42	85	13	13.50	4.85	65	56	13
Ave. L.T. of second quarter.	22.95	12.50	41	42	7	11.35	2.26	71	36	7	33.69	16.15	52	49	7	27.87	2.93	89	16	6
Ave. L.T. of all.....	14.43	9.83	32	139	27	5.76	1.87	67	126	27	19.42	10.56	451	80	27	14.88	2.68	82	88	25
A.D.....	4.92	5.17				3.40	.43				7.65	4.58				6.88	.96			

Q.F. = Quickly forgotten pairs.
S.F. = Slowly forgotten pairs.

TABLE III

	S.B.							R.C.							W.S.							W.P.						
	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2
First quarter	26	11	1	1	37	32	33	11	7	2	1	31	28	23	35	7	0	0	34	31	24	46	2			16	9	7
Mid. half	48	23	6	5	71	61	43	42	26	3	3	82	59	56	62	21	6	2	56	39	33	85	26	2	1	56	40	34
Fourth quarter	27	5	1	1	41	37	33	18	11			36	31	26	42	7	2	1	36	27	19	46	16	3	3	16	11	14
Totals	101	39	8	7	149	130	109	71	44	5	4	149	118	105	139	35	8	3	126	97	76	180	44	5	4	88	60	55

a_1 AND a_2 EXPRESSED IN PERCENTAGES

	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2	Q.F.	a_1	a_2	af	S.F.	a_1	a_2
First quarter	26	42	4	4	37	87	89	11	64	19	9	31	90	74	35	20			34	91	71	46	2			16	56	44
Mid. half	48	48	12	10	71	86	60	42	62	7	7	82	72	68	62	34	9	3	56	70	59	48	31	2	1	56	71	61
Fourth quarter	27	19	4	4	41	90	80	18	61			36	86	58	42	17	5	2	36	75	53	49	33	6	6	16	69	87
Totals	101	39	8	7	149	87	76	71	62	7	6	149	79	70	139	25	6	2	126	77	60	180	24	3	2	88	68	62

a_1 = No. of associative aids, first day.

a_2 = No. of asociative aids, second day.

af = No. of associative aids working falsely on second day.

Under Q.F. and S.F. are stated the number of pairs in each quarter or half.

TABLE IV

	Q.F	S.F.
General average L.T. first day	12.41	7.63
General average L.T. second day	8.34	2.31
Per cent. saved	32	69
Number of subjects	27	27
Number of pairs	491	512
Per cent. of pairs with A_1	33	79
Per cent. of pairs with A_2	5	67
Per cent. of pairs with Af	4	0

with associative aids on the first day. The per cent. varies from 39 to 62 for the Q.F. pairs and from 68 to 87 for the S.F. pairs. Second, we notice that on the second day most of the associative aids for the Q.F. pairs are forgotten and nearly all of those that are not forgotten produce a false response; but most of the associative aids for the S.F. pairs are remembered. The per cent. of the associative pairs for the Q.F. pairs remembered varies from 3 to 8, and from 2 to 7 per cent. of them produce false responses. But the per cent. of associative aids for the S.F. varies from 60 to 76. The meaning of these facts is quite clear, but it may be made still more clear by a study of the general averages, totals, and per cents. in Table IV.

In Table IV. I wish to draw attention to the much smaller L.T.'s for the S.F. pairs, then to their much greater retention, then to the approximately equal number of Q.F. and S.F. pairs, then to the fact that more than twice as many of the S.F. pairs are learned with associative aids on the first day and more than 13 times as many of them are learned on the second day, and finally to the fact that four fifths of the associative aids for the Q.F. pairs remembered on the second day produce false responses. I believe that these facts establish a causal relation between the rate of learning and forgetting on the one hand, and associative aids on the other hand. This is confirmed positively by the high frequency of the latter in the S.F. pairs, and negatively by their low frequency in the Q.F. pairs. We also see that although the Q.F. and S.F. pairs are learned in qualitatively different ways, yet this qualitative difference is stateable in quantitative terms in the number of associations.

Confirmation of the above conclusions as well as suggestive evidence of why there is not a 100 per cent. frequency of associative aids in the S.F. pairs is obtained by calculating the number of promptings, R., required to learn the Q.F. and S.F. pairs as this is influenced by the presence and absence of associative aids. The results are given in Table V. From this we see that the presence of associative aids

TABLE V

	S.B.	R.C.	W.S.	W.P.
Ave. No. of R. for Q.F. pairs with associations.....	2.14	1.78	2.58	3.56
Ave. No. of R. for Q.F. pairs without associations.....	3.15	2.48	3.57	4.92
Ave. No. of R. for S.F. pairs with associations.....	1.51	1.44	1.65	2.93
Ave. No. of R. for S.F. pairs without associations.....	2.42	1.92	2.84	4.66

makes a considerable reduction of the number of prompting in both the Q.F. and the S.F. pairs, that is, they make learning easier. The greater number of promptings for the S.F. pairs without association suggest that high retention may be due to strong imprinting as well as the presence of an association. Because of this factor we would not expect a 100 per cent. frequency of associative aids in the S.F. pairs or 100 per cent. absence in the Q.F. pairs.

Having stated the general principles about the relation of associative aids to the rate of learning and of forgetting, it may be well to reproduce a few of the original records as illustrations both of the rules and of the possibility of treating the data statistically.

Miss Bar. 3:15 P.M. '15.	March 5	March 6
	6. clean 1″	
	5. No 7″	
	4. space 5″	
	3. No 7″	
	2. simper 3″	2. clean 1.5″
Mistake clean	1.	1. No 5″
First day	No. assoc.	
Second day	No. assoc.	
	2. stencil 4″	
Kimono stencil	1.	1. stencil 2″
First day	Thought of design stenciled on kimono.	
Second day	Thought of design stenciled on kimono.	

Miss Hill. 5 P.M.	May 4	May 5
	2. Tomato 1″	
Cheese tomato	1.	1. tomato 1.1″
First day	Both used in rarebit	
Second day	Both used in rarebit	
		3. blast 1.0
	2. blast 2.6″	2. No 3.6
Troll blast	1.	1. betide 1.7
First day	No assoc.	
Second day	No assoc.	
Miss Bar. 3:15 '15.	March 5	March 6.
	4. attack 4″	4. attack 3″
	3. No 5″	3. No 5″
	2. No 6.5″	2. No. 5″
Angriff attack	1.	1. No 1″
First day	No assoc.	
Second day	No assoc.	
	2. lemon 2.5″	
Citrone lemon	1.	1. lemon 2″
First day	Thought of something sour and a response to a three-syllable word.	
Second day	Same as yesterday.	
Miss Boss. 4 P.M., '15	May 25	May 26
		3. gom 6″
	2. gom 2.2″	2. niff 5″
Vif gom	1.	1. lif 5″
First day	Thought of folks chewing gum and coming down with wif.	
Second day	Nothing before first two promptings and then I thought of some one's mouth coming down with a vif in chewing gum.	
	4. ruc 7.4″	
	3. No. 6″	
	2. jog 4.″	
Zis ruc	1.	1. ruc 4″
First day	Thought of seeing the rock.	
Second day	Thought of seeing the rock.	

The above examples give the number of promptings, responses, and L.T.'s, and associative aids, if any, for the pairs and subjects named for the first and second days. The first prompting, 1, opposite the pair was the reading of the pair by the experimenter and had no response. Above it are the results of the second, third, and fourth prompting, as the case may be. Under the line containing the word-pair are described the associations if any for that pair, and

the date of the same. The individual records are of course not as regular as the averages but nevertheless show the rule.

It will now be in place to discuss the experiments of other investigators whose results bear upon the conclusions stated above. The first experimental results bearing upon the relation between the rate of learning and of forgetting were obtained by Müller and Schumann. They are given by Ebbinghaus, 'Grundriss,' I., p. 682, and are directly in agreement with the results of Table I., showing that the number of repetitions in relearning and the per cent. S. vary directly with the number of repetitions in learning. The conclusion drawn was that slow learners forget slowly. But we have seen that this depends altogether upon how we measure retention.

Following Müller and Schumann, Meumann made some experiments which he says justify the same rule. He does not give his data, but explains the relationship in terms of the adaptability of the learner's attention, his purpose or adjustment, his imagery and method of learning. The slow learner has fixed attention, steady purpose, visual imagery, and makes little use of secondary characteristics in the impressions. The quick learner has a highly adaptable attention, temporary purposes, auditory imagery, and makes use of secondary means, such as "special auditory and visual remembrances derived from non-essential circumstances which attend the learning."[1] He states that the more extensively we use the latter, the more transient is attention. This is just the opposite of our experimental results, and since Meumann does not give the facts for his statement, I shall make no further answer. I have made no extended study of the type of imagery upon learning, but some of my most rapid learners were so-called visual learners and some of my slowest learners were so-called auditory learners. The associations of the former were such as: "I saw the words" or "I saw the objects." The associations of the latter were such as: "I thought of corporal for 'Körper-body.'" On the other hand I have evidence which apparently confirms Meumann's state-

[1] 'Psychology of Learning,' tr. by Baird, p. 177.

ments on the type of imagery. My own observations lead me to believe that the rate of learning depends upon something much more fundamental than the type of imagery, namely, inherited ability and association. I should agree that the rapid learner has a more adaptable attention than the slow learner, but it is not usually concomitant with rapid forgetting.

Balaban[1] obtained some interesting results upon the influence of association upon the rate of learning and of forgetting. His subjects learned series of 20 unrelated word-pairs. They were instructed to learn one half of them logically and the other half mechanically. To test the influence of these words upon learning, the series were presented once and then tested by the Treffer Method. The logically learned series gave an average of 33.9 successes, and the mechanically learned series an average of 7.9 successes. To test the influence upon retention, the series were completely learned and tested after intervals of 3 and 6 days. Subject I. learned the logical series in 3 repetitions and had 3.3 successes. Subject II. gave similar results. Although the associations in this experiment were largely voluntary, whereas in mine they were largely involuntary and spontaneous, the results of both lead to the same conclusions.

Busemann[2] made groups tests on school children by the Treffer Method by reading to them four series having eight pairs of words from each of the following: concrete nouns, abstract nouns, adjectives, verbs, and meaningless words. The per cent. of successes for each of these were 80.5, 70, 51.7, 49.2, and 14, respectively. For 6 subjects who followed the method of complete learning, the results were:

	Nouns	Adjectives	Syllables
Saving method repetitions to learn	8.5	11.0	13
Per cent. S.	64	57	61
Treffer method R. to learn	10.5	15.1	22
Successes	65	55	41

[1] *Zeit. f. Psychol.*, 56, 1910, 356 ff.
[2] *Zeit. f. Angew. Psychol.*, V, 1911, 211 ff.

Busemann investigated the frequency of associative aids in one subject and found the following: nouns 88 per cent.; adjectives 57 per cent.; and syllables 54 per cent. It will be seen that this experiment in every way confirms the results and conclusions of the present experiment.

G. E. Müller[1] made a study of associative aids in connection with the remarkable memory of Dr. Rückle, who had a memory-span for 72 numbers, and found that he made use of many ingenious aids that would not occur to an ordinary person. For example:

841 suggested 29^2,
295 " 5 times 59,
635 " 5 " 127,

and that 127 is characteristic since it is the first prime number after 113. 535 suggested .535, the side of a circumscribed polygon. 548519 suggested that the difference between the first and the second halves is the prime number 29. 70128 suggested 701 plus 28 = 729 = 9^3. 901 suggested the year in which the Queen of Saxony came to the throne. Müller, after a careful study of these associations, concludes that he is justified in setting up as a general law that exceptional memory performances of this sort are conditioned by the coöperation of natural helps in so far as they do not rest upon mnemonic devices (p. 236). After reviewing the results of the experiments of Balaban, Busemann, Michotte and Ransy, and Ephrussi bearing upon associative aids, he notices that they shorten the learning time, heighten retention, and shorten the reaction time, and concludes that they have the advantage of strengthening the connections between members and of facilitating the transitions between complexes. On the other hand, they have the disadvantage of sometimes diverting the attention from the associated members and of leading to a reproduction of the aid instead of the associated members.[2] So far as my experiment goes, I have no grounds for dissent from G. E. Müller. If spontaneous verbal associations lower the learning time and heighten the retention of

[1] *Zeit. f. Psychol. Erg-Bd.*, 5, 1911.
[2] *Zeit. f. Psychol. Erg-Bd.*, 8, 1913.

verbal material, it would be the same psychological result if mathematical association of Rückle's type should increase the memory-span for numbers, facilitate rapid calculation, and produce high retention for numbers.

Among American investigators who made experiments, the results of which bear upon the relation between the rate of learning and of forgetting, are Whitehead, Henderson, Ogden, Pyle, Norsworthy, Woodworth, and Lyon. Whitehead studied the effect of visual and auditory modes of presentation upon learning and retention, but with regard to the rate of learning he concludes that the slow learner relearns in a shorter time and retains a larger amount than the fast learner. But Pyle, who recalculated his data after eliminating the results of one S. as erroneous, finds by adding the L.T.'s of the fast six and of the slow six that the six who learned in the shortest time also relearned in the shortest time.[1] Henderson, who had subjects read twice a selection from Irving's 'Dutch Homestead' and then had them write down what they could remember after zero, two-day, and four-week intervals, found that the quick learner recalled greater percentages of what they had learned than the slow learners. Ogden,[2] who studied the influence of the rate of reading upon learning and retention, found that the fast learners usually require less time for relearning than the slow ones, and that this is true both for meaningful and nonsense material. Pyle,[3] who had college students memorize 40 items from a book on nature study and relearn them after 24 hours, found that the quickest of four subjects learned the material in 4.1 repetitions and reproduced 38.5 ideas; and that the slowest required 4.7 repetitions and reproduced 37.5 ideas. Two other groups of four subjects each yielded similar results. From 6 subjects who learned series of 25 nonsense syllables forward by the method of daily repeated learning until permanent retention was obtained, and then learned them backwards by the same method, he found that the total number of repetitions required for permanent reten-

[1] Summarized from Lyon, *Arch. of Psychol.*, 1916, No. 34.

[2] *Arch. f. d. ges. Psychol.*, 1904, **2**, 93 ff.

[3] *J. of Educ. Psychol.*, 1913, **2**, 310 ff.

tion in each case was in proportion to the number of repetitions required to learn the series on the first day. According to Lyon,[1] Gamble, who read series of letters, words, and figures to 350 college students and then tested retention by the amounts which they could reproduce after one, two, and five(?) readings, respectively, found a marked correlation between facility in learning and relearning. In her recently reported study[2] of the effect of the rate of repetition on learning and retention, she finds no proof that rapidly repeated series are forgotten more rapidly than slowly repeated series. Her materials consisted entirely of nonsense syllables. Norsworthy[3], who had 83 college students study a list of 1,200 German-English vocabularies 20 minutes a day, 5 days a week, for three weeks and tested retention with selected lists of 50 German words at intervals of 2 days and of 30 days after the third week of study, found that the fastest half of the learners remembered 70 per cent. of the words in the first test and 73 per cent. in the second test. The slowest half remembered only 52 per cent. in the first test and 47 per cent. in the second test. Those who learned over 900 words within the limits of time for study remembered 76 per cent. in the first test and 78 per cent. in the second test, and those learning less than 300 words within the same time remembered 46 per cent. in the first test and 36 per cent. in the second test. The most extensive study upon the problem in question was made by Lyon,[4] who obtained results from 426 subjects, all being adults except 40. His materials for the most part consisted of series of 20 digits, series of 20 nonsense syllables, a series of 20 unrelated 3-letter words, a passage of prose having 100 words, and a passage of poetry having four 4-line stanzas. All of the materials were completely learned, and retention was tested after various intervals first by the amount which the subject could reproduce without a repetition, Method 1, second by the amount which the subject could reproduce after one repetition,

[1] *Op. cit.*, p. 10.
[2] PSYCHOL. MONOG., 1916, No. 96, p. 99 ff.
[3] *J. of Educ. Psychol.*, 1913, 4, 61 ff.
[4] *Op. cit.*

Method 2, and third by the Saving Method, for which the material was completely learned immediately after Method 2 was finished. Since his material may be roughly divided into meaningful (prose and poetry) and meaningless (all the rest), I shall give sample results of each kind from Table XI. for 24 normal college seniors:

NONSENSE SYLLABLES

	L.T.	Rl.T.	Per Cent. S.	Meth. 1	Meth. 2
Upper half	5.5′	6.5′	71	26	44
Lower half	15.0′	12.1′	68	21	36
PROSE					
Upper half	14.3′	4.8′	68	59	80
Lower half	27.3′	9.1′	66	46	67
AVERAGE FOR ALL MATERIAL					
Upper half	12.2′	4.3′	61	39	55
Lower half	21.9′	7.2′	66	30	46

Interval between L.T. and Rl.T. is one week for meaningless material and ten weeks for meaningful. From these and other results, Lyon concludes that "the most general statement that can be made is that those who learn quickly remember longest if the material is logical in character. Where the material is 'illogical' and is meaningful by 'motor associations,' so to speak, the converse is true. This, however, has many exceptions, depending upon the method used" (p. 58). From his study of the merits and demerits of the different methods of measuring retention, he concludes: "The different methods give opposite results, and yet in one sense of the word, one method is as 'correct' as another" (p. 56).

Whatever the value of the different methods used by Lyon may be, it is clear from the above results that the quick learners relearn more quickly and reproduce absolutely greater amounts than the slow learners. If a quick learner relearns his lesson more quickly and reproduces more of it than a slow one, it makes little difference from the standpoint of efficiency what the Saving Method tells us about his 'retention.' I ask again, how can a slow learner remember more

than a quick one when he reproduces less, or how can he forget more slowly when he has a longer relearning time? The lengths of the relearning times as well as of the corresponding results of Methods 1 and 2 clearly show the invalidity of the Saving Method as a measure of forgetting. If we accept this, then we must admit that Lyon's conclusions are at variance with his facts which really support the conclusion that I have drawn from my own data. Lyon's statement of his data, however, is similar to the one that I have used in Table I. We have seen that that method is not well adapted for solving the problem in question. If Lyon could have separated the associatively learned materials from the mechanically learned materials, he would have had little difficulty in solving his problem. In this connection, we notice his conclusion regarding logical material which is quickly learned and slowly forgotten. Is this because of the character of the material or is it because the prose is usually learned by a different psychological process than are nonsense syllables? We noticed from our results that the character of the material is of little importance in determining the relationship between the rates of learning and forgetting in comparison to the way in which that material is learned psychologically. Meaningful material differs from meaningless only in having a greater number of associative aids, and it is those which make possible Lyon's conclusions regarding logical material.

From this review of the facts of the American investigators, only one conclusion is possible, namely, that quick learning means quick relearning. To my mind this is the same as saying that quick learning means slow forgetting. But wherefore this relationship? It appears that none of the above named investigators considered this question, nor was its answer made possible by their methods which lacked the qualitative side. We have seen, however, that the study of associative aids has furnished a reliable solution.

Summary I.

The relation of the rate of learning to the rate of forgetting depends upon three conditions: (1) the character of the

measure; (2) the character of the learning; and (3) the character of the material. If the Saving Method is the measure of retention, then "Quickly learned, quickly forgotten" expresses the relation. If the amount reproduced per unit of time is the measure, then "Quickly learned, slowly forgotten" is true. The Saving Method is, however, a false measure of retention, for it does not express the efficiency of memory. This leaves only the second statement as true, which may be more accurately expressed as follows: the rate of relearning varies directly as the rate of learning, or the rate of forgetting varies inversely as the rate of learning.

The cause of this relationship is found in the character of the learning as shown in the influence of associative aids. Word pairs are quickly learned and slowly forgotten by means of associative aids. The absence of these associates produces slow learning and quick forgetting.

The influence of the character of the material may be stated as follows: For quickly forgotten pairs, the more meaningful the material, the more rapid the forgetting. For slowly forgotten pairs, the more meaningful the material the slower the forgetting.

A review of the literature upon the rate of learning and of forgetting shows that the facts of other investigators support the conclusion stated above, but they drew opposite conclusions because of an uncritical interpretation of the measure of retention.

II

If associative aids are of such great importance in determining the rate of learning and of forgetting, it will be a matter of interest to study them analytically and find their relation to other associations. Analytic studies of associative aids have been made by Ephrussi, Balaban, Busemann, and G. E. Müller. Since such a study and the resulting classification are matters of interpretation rather than of fact, I shall give no account of the classifications made by these investigators. I have largely followed the demands of my materials, and, so far as possible, have adopted the usual categories for this purpose.

Since the material of the experiment consisted of paired associates, it is possible that such an association should be connected with (1) both words, the stimulus and the response, or (2) with the first word only, the stimulus, or (3) with the second word only, the response. The associative aids were accordingly distributed with reference to these situations. The next step in the classification was to distribute the associative aids of each class with reference to their most outstanding relationship to each situation. In naming this relationship, no account was taken of the learner's opinion, nor was such an opinion asked. The subject having reported the association, it was treated as so much matter to be classified with respect to such relationship as the experimenter could find between it and the situations with which it was connected. These relationships, therefore, do not tell us what psychological factors produce these associations, but only give us some ground for a further inference as to what these factors might be, and on the theory of probability help us to predict their recurrence in the future under similar circumstances. Since a classification of associations with respect to two words is rather unusual, the following table of categories will give examples:

Associations of			
Similarity in sound or sight of	Letters	Zis ruc	'Z—R'
	Syllables	Mistake clean	'Take lean'
	Rhythm	Heb tup	'Head top'
Similarity in meaning of	Contrast	Chest muffin	'The one is large and the other is small.'
	Verbal Habit	Körper body	'Corpus body'
	Coördination	Celery wafer	'Lettuce and crackers.'
	Predication	Simmer tarry	'Both slow.'
	Adjectival relation	Mistake clean	'Clean mistake.'
Thoughts of patterns like	Words	Simmer tarry	'Cemetery.'
	Objects	Ring kitten	'A ring around a kitten's neck.'
Thoughts of	Words only	Troll blast	'Saw the words.'
	Objects only	Ribbon banana	'A pink ribbon beside a banana.'
	Words and objects	Soup wall	'Saw the words and the objects.'
Contiguity	Order and position	Geigen fiddle	'Last pair.'
	Time	mif jas	'Third one I missed.'

TABLE VI

Connected with	Per Cent. of Associative Aids Consisting of															Total Number
	Familiar Words or Phrases Similar as to								Thoughts of					Associations of Contiguity in		
	Sound or Sight of			Meaning in					Patterns Like							
	Letters	Syllables	Rhyme	Contrast	Verbal Habit	Coördination	Predication	Adjectival Relation	Words	Objects	Only Words	Only Objects	Both Words and Objects	Order and Position	Time	
S.B.																371
Stimulus and response		.27	.81		.27	.81	11.34	2.70	.81	29.97	3.24	3.78		31.86	.27	348
Stimulus only		.27				.54		.81				1.62				12
Response only		.54				.27		.54		1.08		.54				11
R.C.																344
Stimulus and response			3.48		.29		26.97		.58	15.08	4.35	10.15	.58	29.58	3.19	327
Stimulus only						.29										1
Response only			.87		2.61					.87			.29			16
W.S.																367
Stimulus and response			16.32	1.63	8.97		4.35		7.88	3.53	4.08	1.90		43.24	2.44	331
Stimulus only			1.90				.27									8
Response only			.54				1.36	.27				5.44				28
W.P.																266
Stimulus and response			28.20						3.76		3.76			40.98	4.13	215
Stimulus only		.37	2.63													8
Response only	1.88	.75	11.28						2.25							43

Table VI. gives the percentile frequencies of the associative aids distributed with respect to the three classes named above and for each series used in the experiment. Some of the outstanding features in this table are: (1) Nearly all of the associative aids are connected with both the stimulus and the response, *i. e.*, with both words of a pair. (2) The category of 'order and position' has the highest frequencies. (3) The second highest frequencies are in the associations of predication and of patterns for English pairs, and in the associations of rhythm for German-English vocabularies and for nonsense syllables pairs. (4) If we except the associations of contiguity, we can say that associative aids connected with words of the mother tongue cluster about some likeness in meaning while those that are connected with some foreign language cluster about some likeness in sensory quality. (5) With respect to associations of order and position we can say that these are less frequent with familiar material than with strange material.

If from these facts we attempt to infer the influence of the quality of an association upon its function, it seems that logical associations reduce the rate of learning much more than sensory ones, and are therefore much more important for this function. The evidence for this statement is proposition No. 3 above and the fact that the one L.T. of the first day for the familiar pairs is 6.29″ and for the strange pairs it is 14.69″. We have seen that associative aids of all kinds are favorable to learning. We now see that the degree of their favorableness varies with their quality, the logical being much more favorable than the sensory.

A word of explanation should be added for the high frequencies for the associations of order and position. The series were learned always in the same order from day to day, and this condition no doubt accounts for their high frequencies.

The next question concerns the relation of associative aids to other associative and reproductive tendencies. By the latter I mean those that interfere with the learning process and cause a wrong response. Since associative aids are also

a source of error, it is possible to determine the frequency of error due to this source as compared with that due to other sources, and therefore the comparative value of these associative tendencies for learning and retention. In analyzing these errors, I have followed the same general plan as for the associative aids, but had to make some omissions and additions of the categories. I omitted the response immediately connected with stimulus only and with response only and have added 'mediate with stimulus,' 'mediate with response,' and 'perseveration.' The mediate associations were frequent enough to demand this, and since they were rather unusual, I shall reproduce some specimens:

Mediate through stimulus by similarity in sound or sight of

Letters:
Wip*f*el-summit; Angri*ff*-attack.
Syllables:
*Be*harren-persevere; *Be*sonder-particular.
*An*regen-stimulate; *An*griff-attack.
Rhythm:
Er*ei*gnen-happen; *Gei*gen-fiddle.
*Gei*gen-fiddle; Er*ei*gnen-happen.
Verbal habit:
Beq-vog; Beckon-nod.

Mediate through response by similarity in sound or sight of

Letters:
Yab-*l*ek-*l*uck.
Syllables:
Ereignen-*hap*pen-*hap*py.
Rhythm:
Dev-*cux*-*coax*.
Paf-*zal*-*jal*.
Verbal habit:
Pork-*cocoa*-*bean*.
Ring-*kitten*-*cat*.

In the above examples, the last word is the response given and the first word is the stimulus. The elements in the stimulus or other word printed in italics are the factors which in the judgment of the experimenter might have led to the response named. For example, Ereignen was often responded to by fiddle, probably because the sound of *eig* occurs both in Ereignen and Geigen. There is no proof that ereignen suggested geigen, and that geigen then suggested fiddle. It is more probable that both happen and fiddle during the

early stages of learning were associated with the sound of *eig*, and sometimes it suggested happen and sometimes fiddle. But this type of association I have called mediate because a certain stimulus suggested a second response whose stimulus had something in common with the first stimulus.

Besides mediate associations, I have added perseveration as a category for the distribution of error because many of the responses could not be explained otherwise. The criteria of perseveration were: (1) repetition of a response on the same day after it had been given at its correct place, (2) repetition of a response stimulus after stimulus because the subject could apparently think of no other and knew that it belonged somewhere in the series, (3) when a response was given to a stimulus with which it had no discoverable associative connection.

The criteria of associations of order and position should also be stated at this point. When two or more responses were given in the correct order but out of position, or when a word or syllable was named one position in advance of its proper place, the errors were classified as due to associations of order and position.

TABLE VII

	Per Cent. of Wrong Responses Due to Association																		Total Number of Wrong Responses
	Immediate with Stimulus and Response by									Mediate with Stimulus by Similarity in Sound or Sight				Mediate with Response by Similarity in Sound or Sight				Perseveration	
	Associative Aids	Similarity in							Contiguity										
		Sound or Sight			Meaning														
		Letters	Syllables	Rhyme	Verbal Habit	Coördination	Adjectival Relation	Predication	Order and Position	Letters	Syllables	Rhyme	Verbal Habit	Letters	Syllables	Rhyme	Verbal Habit		
S.B.	14.90			0.97	1.62			0.32	35.96				.97	.97	.32	5.83	3.24	34.09	305
R.C.	5.55			.46	4.16			1.39	42.59							2.77	7.40	35.18	216
W.S.	3.19		1.66	.49	1.82				28.05	1.99	11.28	2.49	0.16	2.15	.66	2.32	.16	43.49	602
W.P.	3.38	.32		7.72					26.56			1.12	1.28	4.83		30.59	.80	29.78	621

Table VII. gives the results of the distribution of the errors according to their type of association. The noteworthy features of this table are that an average of 35.29 per cent., or little over one third of the errors are due to perseveration. An average of 33.29 per cent. or one third

are due to associations of order and position. The other third of the errors are distributed chiefly among the associative aids and mediate associations. Only 6.78 per cent., or one fifteenth of the errors, are due to associative aids. In all of these cases, the aid was reproduced instead of the associate. There are almost no errors due to logical associations. It therefore appears that the small per cent. of error due to associative aids and the absence of error due to logical associations are negative confirmations of the importance we have attributed to them for learning and retention. While associations of contiguity are important for learning, they produce many errors before becoming fixed. The most important source of error is due to perseveration. That over one third of the errors must be attributed to some such factor as perseveration may be taken as strong proof for its existence and as a confirmation of the evidence of Müller and Pilzeker who named it. Interference from perseveration occurs chiefly during the early stages of learning while the various associations are in process of formation. After associations are once formed and the series learned, their chances of interference with second series are greatly reduced not only because of their fixed character but also because the learning of the second series is directed by a new and different adjustment or Aufgabe. The present experiment also shows evidence of this, for the four series of associates were learned in immediate succession but I can find no errors or responses that were carried over from one series to the next, and on this basis there is no evidence of perseveration. This, however, does not contradict the role which it plays within the series, and I cannot, therefore, accept De Camp's[1] evidence as proof. It should be emphasized that perseveration according to the evidence of this experiment is an inhibitive and disintegrating factor in learning. It produces interference and error and allows their ideas to flow away from their anchors. It is so far uneconomical and destroys the adaptability of the mind while the latter is attempting to do its proper work. It, therefore, appears as a symptom of weakness and inefficiency.

[1] PSYCHOL. MONOG., 1915, No. 84.

But it must be remembered that our experiment furnished no measure of whatever positive benefits it might have.

If we examine the facts of Tables V. and VI. with respect to the frequency of the various types as compared with those for free association, we notice many differences. For example, in Table V. there are no categories for superordination, subordination, cause-effect, verbs, participles, verb-object, word-compounding, etc., such as those used by Woodrow and Lowell. All of these excepting verbs, participles, and word-compounding were in my original table, but none of these types occurred. Those that did occur are distributed very much differently from those in free association, the striking differences being the high frequencies for order and position, patterns, predication, and rhythm. In Table VI. the distribution is still more concentrated, there being only three general types, perseveration, contiguity, and similarity in sensory quality. This is probably due to the fact that associative aids were spontaneous but the responses for the facts of Table VI., excepting the errors due to associative aids, were controlled, *i. e.*, they were selected from the material given and were only misplaced. In a measure, the associative aids were also controlled. They were directed by the Aufgabe to learn the pairs given and had to be adapted to these materials. The Aufgabe and the nature of the materials thus require no correspondence of frequency in types with those of free association.

Summary II.

An attempt to classify associative aids under the usual categories for free association shows a much more concentrated distribution with respect to the number of classes, the principal classes being order and position, patterns, predication, and rhythm.

Their relation to other associations as indicated by an analysis of errors shows that only about 7% of the errors in learning are due to them, leaving 93% due to other reproductive tendencies the most of which may be classified under perseveration, contiguity and sensory similarity.

The distribution of the types of associative aids and of the errors in learning has little agreement with that for free association because the former are a form of controlled association.

So far as the function of associative aids is correlated with their type, it appears that those based on meaning are much more effective in learning and retention than those based on sensory quality, but all types have a favorable influence on mental economy. As such they are distinguished from perseveration, which is a disintegrating factor in the work of the mind.

SIMULTANEOUS VERSUS SUCCESSIVE ASSOCIATION[1]

BY SVEN FROEBERG

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In a recent number of the *British Journal of Psychology* A. Wohlgemuth[2] attempts on the basis of certain experimental results to decide between the rival theories of simultaneity and succession as conditions of association. He argues that, if the former is the true theory, simultaneous presentation should be more favorable for association than successive. If the latter theory is true the reverse should be the case. The results which he obtains are regarded as favorable to simultaneous presentation and the conclusion drawn that association takes place only between experiences which occur simultaneously. Apparent cases of successive association are to be explained as due to simultaneity between the succeeding experience and the disappearance, or 'akoluthic,' phase of the preceding one.

Objection may be made, however, both to the method of the experiments and to the interpretation of the results. In the first set of experiments, in which four observers were used, all give more favorable results with successive presentation, though the variability of the averages, and hence the reliability, is not given. But these results are considered irrelevant and discarded because the exposure time was not the same in the two series. Each member of the pair in the successive series was exposed for the same length of time as the pair of simultaneous stimuli, and hence it was held that the 'learning time' was twice as long in the successive as in the simultaneous presentation. To equalize the time two different methods were used. In one set the exposure time for the simultaneous pair was made twice as long as for each

[1] From the Psychological Laboratory of the University of Michigan.

[2] A. Wohlgemuth, Simultaneous and Successive Association, *Brit. J. Psychol.*, Vol. VII., 434 ff.

member of the successive pair, the number of presentations remaining the same in the two series. In the other set the exposure times were the same as in the original experiments but the number of presentations was twice as great in the simultaneous series as in the successive. The last method must be objected to because it disregards the effect of distributing the repetitions. The first implies that the strength of an association depends upon the exposure time of the stimuli only, regardless of whether or not they are exposed at the same time, and thus contradicts the theory which was to be proved, namely that association takes place only while the two experiences are present together in consciousness. Both methods are, therefore, based upon inadmissible assumptions and must be rejected. But even if the assumptions were admitted, no safe conclusion could be drawn from the results, because the differences in favor of simultaneity are in every case less than the P. E. and in five cases out of the sixteen they are negative. As far as determining whether simultaneity or succession is the real condition of association these experiments prove nothing.

The present investigation is an attempt to repeat Wohlgemuth's experiments with the objectionable features omitted. It seemed also desirable to determine, apart from any theory concerning the conditions of association, the relative merits of the two methods of presentation as well as the effect of increasing the interval between the two stimuli in successive presentation.

The material consisted of seven sets of seven series each of nonsense syllables, each series consisting of five pairs. One set was used for simultaneous exposure, the other six for successive, varying from immediate succession to succession after an interval of five seconds. It was decided to have seven series of each kind of exposure in order that each one might successively occupy the positions one to seven in the daily program and thus neutralize any possible effect of practice or fatigue. For this purpose 245 pairs or 490 different stimuli were needed, and nonsense syllables, though not free from objections, seemed the only material available.

Since Wohlgemuth used only 24 pairs or 48 stimuli, he was quite justified in using other kinds of material. The exposure apparatus consisted of a chronograph in which the pen was replaced by a movable screen with a suitable aperture. The syllables were pasted on a sheet of paper, which in turn was fastened to the drum in such a manner that the rotation of the drum would expose them either simultaneously or successively at desired intervals, and also present the first members of the pairs for a test series. In the latter, the syllables were never presented in the same order as in the learning series, in order to eliminate position as a factor in association. The exposure time varied from ⅓ to ¼ second, but was kept constant for each observer. The interval between the pairs was always 6 sec., between exposure and test 10 sec., and between successive series 12 sec. The arrangement also permitted a variation in the number of exposures of each pair, a condition made necessary by the difference in learning ability of the observers. In most cases only two repetitions were necessary, but in one case three and in another four were used.

The observers, all students in psychology and the majority undergraduates, were instructed to adopt a purely passive attitude, to observe attentively but make no particular effort to memorize the syllables, to refrain from pronouncing them either orally or mentally, to make no special effort to retain them, and above all to refrain from all sense associations. At first it was found rather difficult to follow these instructions, especially with regard to internal speech, but after a few trials these difficulties seemed to have disappeared. Even the tendency to articulate the syllables disappeared except in the simultaneous series, where it persisted to the end. This tendency took the form of combining the two syllables into one word and occasionally reading meaning into it. For example, the two syllables 'dis' and 'mel' appearing together were read as 'dismal.' Whenever the connection of the syllables with an idea was strong enough to be considered a help in recalling the associate the result was not included in the average for that series. Most of the observers had

little practice in introspection and for this reason introspective accounts were asked for mainly for the purpose of seeing how faithfully the instructions were carried out. The introspections are in essential agreement on three points: (1) that simultaneous exposure taxes the attention more than successive, (2) that there is a persistent tendency towards articulation in the simultaneous series, and (3) that when in successive association there was an appreciable interval between the stimuli, the first would remain in consciousness, though with varying degrees of intensity, until the second arrived.

The results from seven observers are presented in the following table in terms of per cent. of right responses, including averages, average deviations (δ), differences between averages, and the P. E. of the differences.

TABLE I.

	Simultaneous		Successive											
			0		1		2		3		4		5	
	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ
Bu.....	67	20	56	22	44	16	53	18	44	22	58	14	46	32
D.....	67	18	51	18	53	26	66	14	62	10	46	22	59	20
G.....	69	22	47	16	52	10	38	10	38	14	28	16	34	18
I.....	52	14	44	14	37	22	24	20	31	20	23	16	37	10
L.....	34	18	23	12	33	18	38	16	40	22	43	16	41	6
M.....	46	14	28	24	25	30	34	20	60	20	43	26	28	18
Br.....	90	14	98	4	92	10	81	20	80	28	87	18	81	24
Av.....	61	18	49	16	45	18	48	16	51	20	49	18	49	18
Diff.....			12		4		−3		−3		2		0	
P. E.$_d$.....			7.7		7.7		7.7		8.2		8.6		8.5	

The difference between the simultaneous and successive series is small, less than twice the P. E. Nevertheless, since the simultaneous exposure yields better results in every case except one, the average difference of 12 per cent. in its favor seems significant. But the expected decrease with increase of the interval between the paired stimuli is not found. Association is made as readily between syllables five seconds apart as between those in immediate succession. The introspections must be resorted to in order to explain these results. These clearly show that the limit of duration of

the primary memory image had not been reached even in the case of the longest interval. One conclusion seems to follow from these results: the effectiveness of the memory image for association does not appreciably decrease for at least five seconds after the stimulus has ceased.

The slight superiority of simultaneous presentation does not necessarily prove the theory of simultaneity; it is more readily explained by the tendency to articulate and combine the simultaneous syllables into a single word.

These results, however, are not quite comparable with those of Wohlgemuth, for this investigator expressly rejects results based upon experiments with nonsense syllables and maintains that material free from the influence of motor speech habits alone give psychological association. Another group of experiments was, therefore, devised in which colors and letters of the alphabet replaced the nonsense syllables. Geometrical figures would have been preferable to the letters, but could not be used on account of the large number needed and the difficulty of making them distinct and yet small enough to fit the apparatus. Twenty-eight colors were used, sufficiently different to be clearly distinguishable. Besides the 26 letters of the alphabet the signs & and $ were used. This gave four series of seven pairs each, two of which were simultaneous and two successive. Twelve other series were obtained by arranging the colors and letters in different combinations. Each pair was exposed twice. The exposure time was ⅓ sec., as before. Suitable arrangements to eliminate the effect of practice and fatigue were made. Other details were the same as in the previous experiments.

The results are given in the following table in terms of per cent. of right responses, including averages, average deviations, differences, and the P. E. of the differences.

Here again the differences between the two methods of presentation are small. In only one case does the difference exceed twice the P. E. But every one of the eight observers gives better results with successive presentation, and it is unlikely that this is due to mere chance. It may, therefore, be assumed that the observers possess the same type of

associative mechanism and that it is permissible to pool the results of the group. When this is done an average difference of 6 per cent., with a P. E. of 1.9 per cent., is obtained in favor of successive presentation. Since the P. E. is less than ⅓ the difference, it is extremely unlikely that the result can be due to chance.

TABLE II.

	Simul.		Suc.		Diff.	
	Av.	σ	Av.	σ	Av.	P.E.$_d$
B.	58	18.0	65	20.0	7	5.3
D.	71	11.2	75	14.2	4	1.8
G.	35	16.0	36	14.6	1	1.8
I.	39	20.6	47	24.0	8	6.7
L.	37	24.8	44	9.6	7	5.5
M.	51	20.4	59	16.0	9	5.3
Ge.	25	14.6	27	12.4	2	4.3
K.	25	16.6	33	16.4	8	4.8
Av.	42	17.8	48	15.9	6	4.2
Pooled results of all					6	1.9

It appears, then, that the results vary with the kind of material used. Nonsense syllables are more readily associated when presented simultaneously, colors and letters when presented successively. The suggestion immediately arises that it is purely a matter of attention in relation to the nature of the material. If the material is such that the pair of stimuli form an organic unit, or can be attended to as a unit, simultaneous presentation is preferable; when such that the members of the pair come as separate entities, or must be attended to separately in order to be apprehended, successive presentation is preferable. If a complex stimulus can be attended to as a whole, simultaneous presentation increases the bonds between its parts; when this is not possible, the part not attended to will be lost and fail to be associated with the rest. The physiological basis of association may be, and very likely is, the same in both cases.

In order to eliminate the persisting memory images during the interval between the two stimuli in successive presentation two-digit numbers were inserted between these. The observers were required to read the numbers aloud and keep their attention centered upon them rather than on the syllables,

except when the latter were exposed. It was planned to have the observers add some constant number to the numbers exposed in case the reading of them should prove insufficient to occupy their whole attention. But this was not found necessary. Every one of the observers reported that the reading of the numbers effectively and completely obliterated the memory image of the preceding syllable. It was thought that by this means a measure of the strength at various points of the neural 'akoluthic' phase could be obtained. In this part of the experiments each stimulus pair was exposed four times. In all other respects the material as well as the procedure was the same as in the first part of the experiments.

The results in terms of right responses are given in the following table.

TABLE III.

	Simultaneous		Successive											
			0		1		2		3		4		5	
	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ	Av.	δ
O.	49	15.4	44	26.3	46	29.2	41	18.8	59	19.4	27	12.2	24	18.8
J.	33	14.0	31	12.6	17	13.4	14	13.4	20	12.2	9	12.0	18	16.0
H.	77	18.0	50	17.2	31	13.2	47	26.0	39	18.8	24	20.2	21	13.8
S.	59	16.6	56	23.2	42	11.2	42	11.8	42	25.4	34	20.8	24	20.8
Av.	54		45		34		36		40		24		22	
Diff.		9		11		−2		−4		16		2		
P. E.$_d$		6.8		5.5		5.6		6.4		6.2		5.5		

There is thus a distinct, though irregular, decrease in the number of right responses as the interval between the members of the pair in successive presentation increases. At the greatest interval used there are still 22 per cent. right responses. It may be argued that association in this case is due not to the waning neural activity set up by the preceding stimulus but to connection with the apparatus or the task. In such a case, however, any syllable of the series should have an equal chance of being recalled, and the chances for a right response should not exceed 20 per cent. If some of the syllables of the series could not be recalled, or, as sometimes happened, syllables from other series were recalled, the chances for a right response on the basis of association with the task alone must be still smaller. The maximum

strength of this factor, therefore, can not exceed 20 per cent., and is probably much less.

It appears, then, that simultaneity of two experiences is not necessary for an association to be formed between them. Nor is it necessary, though helpful, that memory images of the preceding experience be present simultaneously with the succeeding one. An association may still be formed between two experiences where the first has already passed out of consciousness when the second one appears, or in Wohlgemuth's terms, between one experience and the unconscious 'akoluthic' phase of another.

DISCUSSION

MISS CALKINS'S CASE OF SELF AGAINST SOUL

I.

On first reading Professor Calkins's article, 'The Case of Self against Soul' (THE PSYCHOLOGICAL REVIEW, July, 1917) I thought she had brought to light something in Plato that had escaped my notice. When she points out that Plato treats the soul sometimes as the principle of life and sometimes as the self, of course she is on familiar ground; but when she argues that he holds also a 'metaphysical' conception, she makes a point that I had never recognized.

In the end, however, she does not convince me. An indispensable feature of the 'metaphysical' conception of the soul as contrasted with the self conception appears to be that the soul is not conceived as, like the self, essentially conscious. To this she would doubtless assent, but she adduces no evidence that Plato ever wavered in his identification of soul with consciousness. What she does argue is that he treats the soul as immaterial, simple, unchanging; also, incidentally, as imprisoned in the body. Of course the proofs of these points are clear, but they do not show any conception distinguishable from the self conception. In fact, all the evidence appears to me to show that these characters are actually ascribed by Plato to the self.

For the soul is immaterial; but so is the self. The soul is also ἀσύνθετος, *i, e.*, as Plato explains ('Phædo,' 78), not put together out of independently existing entities; but the self is likewise ἀσύνθετος,—Plato's proof of the simplicity of the soul in fact rests on its self-character. And the soul is changeless, ἀεὶ κατὰ ταὐτὰ ὡσαύτως ἔχει ('Phædo,' 79 *D*, etc.), when, acting according to her own proper nature, she contemplates the unchanging ('Phædo,' 79 *C*, *D*), though she can be dragged down into the region of the changeable; so would the self be changeless if it were contemplating only the unchanging, since there could be no possible occasion to change. In the passage just cited the argument for the changelessness of the soul is drawn from her self-character. As for her imprisonment in the body, Plato twice points out ('Cratylus,' 400 *C*,

'Phædo,' 62 *B*) that this is not a doctrine of his originating; when he gives his own interpretation of the 'imprisonment' ('Phaedo,' 65, 66) he puts it wholly in self terms. Thus all the characters that Miss Calkins adduces to show that Plato had a 'metaphysical' conception of the soul are characters ascribed by him (and by her, I should think) to the self.

If one does not attribute to Plato any separate 'metaphysical' conception, the other two conceptions coalesce, and his whole doctrine is harmonious as far as the nature of the soul is concerned. For life-giving character and consciousness appear to have been thought of as, strictly speaking, aspects or functions of one and the same soul. So Aristotle regards them ('De An.,' Bk. I., Ch. II., p. 403 *b*, 26–7), and so, apparently does Plato; life and νοῦς must be in soul ('Sophist,' 249 *A*)—not, however, as separate contents, else he would hardly argue directly from one to the other in the 'Laws' and in the 'Phædrus' myth. As I understand him, the two aspects are inseparable—the self *is* what vitalizes. In fact no special violence would be done to Plato if one were to translate ψυχή steadily as 'self.' Plants, I suppose, were not regarded by him as having a ψυχή that is not a self, for in the only places in which he touches on the matter, as far as I remember, he calls plants animals, or kindred to animals ('Timæus', 77, A, B, 80 *E*). It is presumably with Aristotle that the conception of a merely nutritive ψυχή originates.

Miss Calkins may of course have more or less in mind some other meaning for 'metaphysical' than appears in her article. As she points out, soul-substance as conceived, *e. g.*, by Locke appears to differ from self in that its existence is something over and above its experience. What that 'over and above' is, I regard as irrelevant in case of Plato, because I do not find in him any trace of a belief in it. I agree with her that, as a safeguard against such a conception, the abandonment of the term 'soul' would be of value, even though 'self' is not free from the danger of like misinterpretation. Ultimately I hope 'soul' may come back to its Aristotelian meaning. Hegel has convinced me that there is need of such a word; in spite of fantastic details his treatment of life (embodied soul) impresses me as on the whole one of his greatest successes.

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II

In her article, 'The Case of Self against Soul' (published in THE PSYCHOLOGICAL REVIEW for July, 1917) Miss Calkins finds that one of the main sources of prejudice in regard to a 'self-psychology' is the traditional confusion of self with soul. By means of an historical study she has undertaken to distinguish these conceptions, hoping thereby to aid in removing the prejudice against the self as a legitimate standpoint in psychology. The study, as she says, "culminates in one insistent conclusion: the soul must go. As an historic concept of immense influence, it will always retain its prominent place in the history of ideas; as a term of modern psychology it has outlived any use it may once have had and has become a source of mischievous confusion."

Quite apart from the use which Miss Calkins has made of the results of her study of the classical systems in support of her own point of view, or at least in seeking to gain for it a fairer and more unprejudiced examination, this paper is noteworthy as recognizing that modern psychology can profit by an historical examination of its working conceptions—that, indeed, such investigations are necessary to enable it to avoid confusion and prejudice. It is noteworthy, also, as an example of careful and scholarly work, and the distinctions to which it leads are highly significant, both for psychology and philosophy. I agree with her in believing that in both fields there has been a good deal of confusion because of the identification of the concept of 'self' with the metaphysical (in a bad sense) notion of a 'soul' (or as I have been accustomed to say, a 'soul-substance'). This confusion, I agree in believing, has operated prejudicially in preventing oftentimes both a fair hearing and a clear understanding of the arguments for the concrete notion of a 'self.'

My main purpose, however, in commenting upon Miss Calkins's article lies apart from these considerations. While I agree with her in maintaining that, for some purposes at least, it is essential that psychology should be written in terms of the life of a 'self,'[1] it has always seemed to me that in defending this doctrine she has taken up an indefensible position in insisting that the 'self' is actually a 'perceived' fact, a particular datum of introspection. In the paper under discussion, this question does not come up for explicit consideration; but it is suggested that one explanation of the failure of psychologists to recognize the self is their 'inattention' to a content

[1] Cf. my article, 'The Standpoint of Psychology,' *Phil. Rev.*, 1914, 23, 159–166.

which is 'ubiquitous.' As an example of the tendency to overlook constant or ubiquitous contents, reference is made to the sensations of pressure from the clothing, followed by the statement, 'so in my introspection I simply forget to name myself' (pp. 278–9).

Now I am not sure that Miss Calkins would rest her whole case in regard to the self upon the possibility of discovering through introspection some specific self-content, parallel, in some sense, to the sensations arising from the pressure of clothing. If so, the basis appears to me very precarious. My own introspection would compel me to agree on this point with the exponents of 'psychology without a self.' If under the name of self we are looking for some 'psychological state,' Hume's famous saying that 'we can never catch the self' would seem to me to be the last word. But, apart from introspection, does not the concrete idea of the self forbid any claim to know it as an object? It appears to me that such a claim is intelligible only so long as one thinks of the self as a particular 'thing' or substance, analogous to other things, and differing from them, not in principle, but merely in degree of 'constancy,' or of 'ubiquity.'

But if the self is the self, *i. e.*, the universal subject, surely it cannot be known as an object without losing its character. And is it not true also that its concrete universality cannot be defined or adequately characterized in terms of ubiquity and constancy? What is essential, it would seem, in approaching a study of the mind from the point of view of the self is to adopt a new standpoint, to appreciate and describe in terms of a new category, rather than to discover some new specific content.

I am not sure that this is true of Miss Calkins, but I think that oftentimes the motive in insisting upon the self as an observed fact of observation, is to maintain its unambiguous reality, as something more than an inference, or a mere hypothesis. With that *motive* I entirely sympathize, but, already suggested, I think that it is mistaken in the contentions it employs to support its conclusion. For surely the reality of the self cannot be established by crying, 'lo here! or, lo there!' It is the universal subject of all experience, but cannot be turned into an object by psychology, and pointed at as *the thing that is* '*highly complex*,' '*persistent*,' '*unique*,' *etc.* That is the soul-substance over again under a different name! But, as I think Miss Calkins would agree, a 'self-psychology' is not a psychology which requires us to find some specific content or object named the self, but it is a standpoint which sees the self as

the universal subject of all experience, as the universal category or speculative idea that gives unity and significance to the various elements in the mental life. The self is not substance but subject, not a mere particular 'existence,' but a true individual—something more and other than the type of reality which one can point at or 'hold in the hand.'

But how is the self known? If it is not an observed fact, is it a mere inference; and if so, what facts compel the inference? It seems to me that these questions are relevant only so long as one keeps thinking of the self as a particular fact for psychology, on the same plane as other facts. The reality of the self, one might say, is not a fact or an inference for psychology, but is the basis for all observation and inference in psychology and elsewhere. One cannot prove its reality without using it.

I think, however, that Miss Calkins means that the knowledge of itself which the self possesses is direct, and that in this sense it may be called a fact of observation, and not an inference. Undoubtedly the self knows itself—that is involved in being a self—but is this knowledge simply a 'fact of observation' or introspection? These terms are customarily employed in psychology to denote the direct apprehension of particular contents. When it is asserted that the self, too, is apprehended in this way, is there not danger that its real nature as universal subject shall be obscured, that it shall be regarded merely as a constant and ubiquitous element of consciousness, like the constant sensations arising from the pressure of clothing?

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REPLY

I shall try not to abuse the courtesy of the editor who permits me to reply to the comments of my friendly critics. I am impressed with the strength of the argument which leads Professor Case to identify Plato's 'soul' with 'self'—especially impressed with her analysis of 'Phædo,' 65, 66 and 79 *C*, *D*. But I question her assertion that as the soul is immaterial 'so is the self.' For the self is non-material rather than immaterial; immateriality is incidental not essential to it. And the considerations urged by Miss Case are not, in my opinion, entirely incompatible with the view that Plato wavers between, and does not identify, 'self' and 'conscious soul.' (For doubtless Plato's soul is conscious as well as immaterial and lifegiving. This is expressly stated in 'The Case of Self against

Soul.') But it is beside my present purpose to consider further the topic, in itself of great interest, of Plato's soul-doctrine and the misconceptions of it which Miss Case attributes to me. For, as she does not fail to point out, the main contention of my paper—that the self must be distinguished both from life and from a merely 'immaterial' soul—is unaffected by the failure or the success of my interpretation of Plato.

Professor Creighton does not, I think, fully realize the extent of my agreement with him. In his admission that we directly know the self as an 'unambiguous reality,' as 'more than an inference' and 'more than a hypothesis,' I find the essentials of my view. I believe with him that the self is not adequately defined in terms of ubiquity and constancy,' for I hold that, as *sui generis*, it is essentially indefinable though (incompletely) describable. And I further agree with Dr. Creighton that the self is not 'a perceived fact,' not a 'content,' not a 'psychological state.' It is not properly called a 'content' or a 'state' because both terms imply fragmentariness and momentariness. And my introspection, like Creighton's and Hume's, discovers no sensational or affective complex constituting a 'specific self-content.' (My comparison, it should be noted of common inattention to the self with inattention to 'sensations of pressure from the clothing' is in no sense intended as a complete analogy between the self and any group of sensations.) But though the self, as basal to all other psychic facts, certainly is not 'on the same plane' with them, I hold that it is none the less a fact in its own right and on its own deeper level. It is a fact because, as Dr. Creighton also believes, it is directly known, not merely inferred. And, as known, it is properly called an object.

Dr. Creighton makes only one positive statement about the self-of-psychology with which I find it difficult to agree. To call the self a 'universal subject of all experience' seems to me a metaphysical, not a psychological, assertion unless the words refer only to the individual experience, meaning simply: wherever there is consciousness a self is being conscious.

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